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Perseveration and the Rate of Recovery After the Psychogalvanic Response

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PERSEVERATION AND THE RATE OF RECOVERY
AFTER THE PSYCHOGALVANIC RESPONSE

By

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A Dissertation Submitted in Partial Fulfillment
of the Requirements for the Degree of Doctor
of Philosophy in Loyola University

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1949

VITA

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CHAPTER I

INTRODUCTION

The study of personality and character constitutes one of the most interesting fields of psychology from both the theoretical and practical standpoints. The interplay of the various observable characteristics of the human person is astonishingly complex, and at no time do we find this complexity more marked than when we apply ourselves to the task of distinguishing various individuals for various degrees and nuances of this or that characteristic. It is true that the attempt to compare individuals in regard to the possession of specific traits has not been very successful on the whole, and that the general outcome of research along these lines has been to show that a single trait does not have meaning in itself, but only in relation to the total personality (136: 846, 865.) It has become increasingly apparent that the all-important thing is the internal guiding and unifying force, which is the self, possessed of various physical, psychophysical, and mental characteristics which it is able to mold and organize through its two highest and most important functions: intelligence and will. Aside from any philosophical considerations, this is becoming increasingly clear from the clinically and experimentally demonstrated importance

of goals and motives both for understanding the individual and aiding his adjustment (129: 213-231).

Despite this fact, it appears also true that quite a bit may be said for the influence of constitutional or physical factors in molding the personality. Such factors are usually conceived of as forming some sort of unity, which may be designated as temperament. In this conception, certain characteristics have their roots in constitutional qualities, which conspire together to form various combinations among which we may recognize typical constellations. These typical constellations, perhaps because of the understandable tendency to economy of thought, have usually been reduced to a few basic traits which are susceptible to estimation or measurement in terms of more or less.

There have been many typologies. In this connection, the modern era of experimental psychology has distinguished itself from the past eras of speculative psychology, not by being slower and more cautious to generalize, but by being decidedly bolder. Witness the number of typologies that have been proposed since Wundt (79: 189-205). At any rate, out of the background of typology, thanks principally to the work of two schools, has emerged the temperament or personality trait called perseverance. Originally discovered by a psychiatrist, Neisser (58) and the experimental psychologists Mueller and Pilzecker, (57) it had the good fortune of falling among men who planned to give

their typology an empirical and even experimental basis. From them it was adopted by a still more experimentally minded group. Neither group, however, was modest in its claims for the protégé.

Indeed, such have been the claims made for perseveration as an important factor in temperament, personality, and character that they cannot wisely be ignored. It has been said, for example, by Spearman that perseveration as he conceives it is

the greatest of all faculties, if by this may be signified the one which has been the most lavish of promises for individual psychology ...

Turning to the practical standpoint, the prospect here is extraordinarily hopeful. When once the pack of modern investigators can be called off the many false scents of illusory faculties to this genuine trail; when the perseveration, already measurable even by groups, has been evaluated for persons of diverse age, sex, character, and social status; when the connection has been traced out which it bears to success in different branches of education and varieties of vocation -- then perhaps psychological science will have made a second advance not much less in magnitude than that which is being achieved with respect to "intelligence". (77: 306f.)

Similar claims have been made for a group of simple tests which have been thought to measure perseveration. However, in fairness to Spearman we must mention that in his latest major work he no longer makes such sweeping claims. In general he speaks cautiously of a P factor which may be perseveration or mental inertia, but even at that he makes the following statement:

On the whole, however, the results appear to be astonishing enough. The tests of P are to all appearances most trivial; they only consist of such performances as writing e's backwards. Nevertheless, as we have seen,

they show themselves to tap the deepest strata of human character. (79: II, 269)

As a result of the direction perseveration research has been given by the theories and the personal guidance of Spearman, attention has been centered primarily on a group of paper-and-pencil tests which are supposed to be indicative of perseveration. These so-called 'p' tests have rather preempted the field, almost to the neglect of the phenomena to which this characteristic owes its name. These phenomena, the recurrence to mind of tunes, words, phrases, ideas, feelings, and so on, in a spontaneous, purposeless fashion, possess interest in their own right. The same may be said of the persistence of a sensation, thought, or mood, and also of the difficulty we sometimes experience in making rapid mental shifts, as when we are suddenly called upon to interrupt one train of thought and take up another. These also seem to be allied to the recurrence manifestations.

The validity of the paper-and-pencil tests referred to by Spearman and of certain tests of sensory perseveration is at best questionable. This will be made abundantly clear in our review of the literature. In view of this fact, the more fruitful approach to the study of perseveration, it seems to the present writer, is to start with the consciously experienced phenomena of persistence, recurrence, and interference of thoughts, moods, etc. That these things occur and that they possess a certain degree of similarity among themselves can hardly be

doubted. Whether or not they are all really connected in such a way as to form a functional unity cannot, however, be assumed but must be examined empirically. This may be done by the familiar statistical methods used in test construction. Some form of questionnaire appears to be the only likely way of getting at and measuring these conscious phenomena.

A few such questionnaires have been constructed and used in perseveration research, but they have all suffered from lack of adequate analysis, or else have been validated against the paper-and-pencil tests as a criterion. This latter practice seems to the present writer entirely fallacious, since it means judging the more certain by the less certain. The criterion should rather be the perseverative phenomena which are manifested introspectively.

A comprehensive attack on the problem of the validity of the perseveration tests and on the problem of the relation of perseveration to the nervous system would be desirable. Nevertheless, since such attempts have been made with inconsistent results, a piecemeal attack suggests itself as advisable. Aside from the prodigious amount of time required for an over-all approach, attempting too much is likely to be conducive to less exact experimental procedures. For this reason we have selected a limited field of investigation. After construction and standardization of a questionnaire for perseveration as observed

introspectively,* we have investigated the relation of this type of phenomenon to the activity of the autonomic nervous system, in order to see if to recurrence, continuance, and interference phenomena consciously experienced there corresponds a tendency for prolonged activity of this branch of the nervous system. The reason for this selection will be given later when we define the problem more carefully. We must first review the previous work done in this field.

* For the sake of brevity we shall refer to this as introspected perseveration. The term is Cattell's (9). Though rather clumsy, it allows us to avoid frequent circumlocutions.

CHAPTER II

THEORIES OF PERSEVERATION AND METHODS OF MEASUREMENT

Theories

One can say that the theory of perseveration has a long or a short history, depending on how far he wishes to look for analogous ideas and what precise modern theory he takes as an analogue. Spearman has, and quite correctly, found an adumbration of his mental inertia theory in Aristotle, (2) together with a mention of one of the perseverative phenomena:

Those feel the vexation most who happen to have fluid in the region of the sensory organ, for once the fluid substance is set in motion it is not easily brought to rest until the object sought for returns to mind and the process resumes its direct course. Hence, when they have set something in agitation, emotions of anger and fear, owing to the reactions of these organs, do not come to rest; on the contrary they react once more on them. The phenomenon resembles that which occurs when a name or a tune or a sentence has come to be much on one's lips; after one has stopped, and without one intending it, one is prompted again to sing or to speak.*

And an appreciable list of names can be compiled if one wishes to search among the writings of the philosophical psychologists

* I have used the translation as given by Spearman (79:II, 59). That of J. I. Beare and G. R. T. Ross in the Oxford series differs from this in detail and is more interpretative; the Greek is not altogether clear in parts.

from the Renaissance to the dawn of experimental psychology. (70).

The term perseveration apparently owes its origin to Neisser (58), who in 1894 called attention to the pathological symptom which consists in the useless and inappropriate repetition of spoken or written words as well as the repetition and continuation of motor activities. The term has been adopted by psychiatrists quite universally, and it is this meaning that is affixed to the term generally in the psychiatric literature.

Though not the first to remark perseveration among normals and in normal mental activities, Mueller and Pilzecker, (57) were the first to discuss the phenomenon at any length and to attach to it any marked theoretical importance. In their very thorough and comprehensive memory experiments, they noticed a tendency for responses from one list of syllables to intrude themselves during the learning of a following list and to cause errors. These syllables came to mind rather spontaneously and compulsively and were on the lips before the subject, realizing their unsuitability, could check them. To explain this phenomenon, these workers postulated a "perseverative tendency," which is something distinct from the mere process of association. They proposed the following hypothesis:

Every presentation, after its entry into consciousness, possesses a perseveration tendency, that is, a tendency, which generally fades quickly, to mount freely

into consciousness. This tendency is the stronger, the more intensively the attention has been directed to the presentation, and increases if the respective presentation or series of presentations is repeated very soon.*

Besides further delimitation of the conditions in which this tendency is operative, they pointed to the after-images of sensation, hallucinations, a similar tendency in the motor field, the psychopathic type of perseveration, as well as related phenomena in the normal ranges such as the recurrence of melodies to mind. They also saw individual differences in perseveration tendency and suggested a relationship with character and significance for education and vocational choice. Furthermore, they maintained that this tendency aids the continuity of thought and plays an essential role in man's higher activities, in which association does not suffice, since it is adequate only in the realm of sensation and the quest of sensory needs.

It is clear, then, that the concept of perseveration as a general function of mental life was introduced to modern psychology by this early work of Mueller and Pilzecker. They did not, however, bring the matter to definite experimentation, except in as far as their assertions in regard to the tendency

*Translation mine: the German is as follows:

Jede Vorstellung besitzt nach ihrem Auftreten im Bewusstsein eine Perseverationstendenz, d.h. eine im Allgemeinen schnell abklingende Tendenz, frei ins Bewusstsein zu steigen. Diese Tendenz ist um so staerker je intensiver die Aufmerksamkeit auf die Vorstellung gerichtet war, und steigert sich, wenn die betreffende Vorstellung oder Vorstellungsreihe sich sehr bald wiederholt. (57: 58)

of presentations to remount into consciousness were based on their data from the memory work with nonsense syllables; the rest was a matter of generalizing further on the basis of similar manifestations observed more or less casually. Several other German workers have followed their lead in investigating this tendency as manifested in memory work, but since this is only a limited aspect of a much more general question we shall not extend our review of the literature to include their work. Foster's article in 1914 (29) will serve as a guide to this field for anyone interested, while Ach (1), Kuehle (52), and Passarge (60) will provide information on later work in Germany. The suggestion that perseveration may have practical import for vocational choice has also been the occasion of some research. Burri (6) and Zillig (95) will serve to introduce one to this field.

The theories of Otto Gross (31) have been important in stimulating research on perseveration. Building up a typology from observation of the two most contrasting mental diseases mania and melancholia, he distinguishes the broad-shallow and the deep-narrow types. The roots of these differences in type he finds in the characteristics of the nervous system by which the first type is dominated by what he calls "primary function," the other, by "secondary function."

Each nervous element whose functional excitement means the occurrence of a presentation in consciousness, persists after the presentation has quitted the span of

consciousness. That is to say, it remains for a further long period in a state of after-function. This after-function ... remains regulative of the further direction of associative activity.*

Heymans, Brugmans, and Wiersma (34, 35, 36, 92, 93), who constitute the Dutch school (so-designated by Spearman), accepted and developed the concepts of primary and secondary function. Perseveration was a term that they used as synonymous with secondary function. It is quite clear that secondary function and also perseveration is in their theory a valuable possession. To this point we may quote a passage from a lecture delivered by Wiersma at the University of London:

We distinguish for each content of consciousness a primary and a secondary function. The primary function is the working during the time that it remains in consciousness: the excitation of images, the formation of associations ... etc. The secondary function is the after-effect, that is the effect on the consciousness when it is no longer above the threshold. Without this after-effect it would be impossible to follow a demonstration, to understand the contents of a book, or to solve a problem. For in consequence of the conception of the problem on the background of the consciousness images arise, which may be of service in solving it, even if the conception of the problem is not actually the subject of thought. (93: 6)

* The translation is that of Spearman (77: 44). The German is:

Jedes nervoese Element, dessen functionelle Erregung das Bestehen einer Vorstellung im Bewusstsein bedeutet, verharret nach dem Austreten dieser Vorstellung aus der Bewusstseinsenge, also nach dem Ablauf seiner eigentlichen Function noch laengere Zeit im Zustand einer Nachfunction unde diese Nachfunction ... ist ... maassgebend fuer die weitere Richtungs-nahme der Associationsthaetigkeit. (31: 10)

In any individual the primary function may be strong and the secondary function weak, so that his activities are relatively dominated by primary function; in another, secondary function may dominate. Thus the relative dominance of primary or secondary function constitutes one of the dimensions of temperament. With this and the dimensions of activity and emotionality, eight basic temperaments are constituted, according to the various possible combinations of extremes along these dimensions. For example, the sanguine person is nonemotional, active, primary functioning; the phlegmatic: nonemotional, active, secondary functioning (36: 51, 1-72). The pathological extreme of the dominance of primary function is mania; of secondary function, melancholia. It is this aspect of their theory that led to the work with the sensory and motor tests, which later became the consecrated tests of perseveration. But we shall discuss that matter later.

With the theory of Lankes (53), the first worker to put to experiment the precise problem of the unity of function amid the various phenomena called perseverative, we shall at present be brief, since he worked under Spearman and probably derived his concept of perseveration from him. For Lankes, perseveration is a "native quality of the nervous system, innately different with different individuals." (53: 418) Though it manifests itself generally in human activities, it is not a determiner of character; the innate tendency of perseveration

can be controlled by the individual. The important advance for theory made by Lankes was the demonstration that greater perseveration does not make for better character. To this point we shall also return later.

By far the most important man in the work on perseveration is Charles Spearman. Much of the actual research was done under his direction or by men who had come under his influence, and of course the impetus was given by his attempt to provide a basic theory that could be verified experimentally and that would determine the place of perseveration in relation to the other factors of mental life. Though his theory was elaborated earlier, we shall follow his presentation of it in The Abilities of Man, where it is described most fully and systematically.

Among the quantitative laws relating to 'g' we find two of interest to us here: the law of retentivity and the law of inertia. The law of retentivity of disposition reads: "Cognitive events by occurring establish dispositions which facilitate their recurrence." (77: 271) The law of inertia or lag reads: "Cognitive processes always both begin and cease more gradually than their (apparent) causes." (77: 291) The first law pertains to memory and to repetitive phenomena, which are not to be confused with inertia or perseveration. The latter is a "unitarily functioning factor" second only to intelligence in importance. It is distinct from steadfastness of purpose,

to which it is somewhat opposed. Perseveration varies independently of 'g'; 'g' is the quantity of mental energy, perseveration is its degree of inertia. The lag of one activity does not necessarily interfere with a subsequent activity; in fact, it does so only in certain cases, as when the two activities conflict or cover each so extensive a field that the required shift is an elaborate operation. An example of the first case is found in the motor perseveration tests, while changing from one school lesson to another would exemplify the other case (77: 291-307).

This theory may appear to be highly speculative, a mere uncritical transfer of the concept of the inertia of matter to the realm of psychic events without empirical foundation. On the contrary, it is an attempt to explain facts which Spearman regarded as adequately proved experimentally and which he carefully reviewed in his book. It is rather a token of the genuinity of his desire to base his theory on facts that he was willing to admit subsequently that contradictory evidence had thrown doubt on his theory.

The first indication of his fair-mindedness in this matter was the admission that Hargreaves and Wynn Jones had shown that some of the alleged perseverative phenomena were really a loss of fluency, and that, while fluency and perseveration seem to be affected together in manic and melancholic states, they are independent in normal conditions. He suggests

that 'p' may measure the mobility of the psychophysical energy. Hence the ambit of the perseveration factor seems to have been pared down, but the fundamental concept of mental inertia seems to remain (78).

In his Psychology down the Ages (79: II, 261), Spearman retains essentially the same theory of perseveration, though he is a bit clearer in delimiting its meaning and its relation to other factors. It is a general, not a specific factor:

However, this explanation of the specific factors we have been considering seems not to extend to O and P. These, in fact, are not rightly termed "specific" at all. They appear rather to be as general as G itself, but in other dimensions. If G is taken to measure the amount of any person's supply of G, then O may represent the instability of that supply, whilst P may represent its inertia in switching from one set of engines* to another. (79: II, 261)

Though he appears here to apply the inertia concept, only to the case of shifting, on the preceding page he speaks of inertia in terms of slowness in starting and stopping. The justification for identifying 'p' with perseveration he seeks in the 'correlation between 'p' and difficulty in getting down to work' observed by Bernstein (77: 260). He admits, however, that his interpretation of 'p' as mental inertia has been challenged and that there are great difficulties of measurement.

* The "engines" are the specific abilities or factors; they are group, not general factors. (*Italics Spearman's.*)

Still, he is of the opinion that perseveration tests "tap the deepest strata of human character," such as self-control, perseverance, and reliability (79: 269 f.).

The outstanding authority on the design of the 'p' tests and their detailed interpretation is probably Stephenson. Though he had at first apparently subscribed to the Spearman theory of mental inertia (80), he does not seem to have at any time looked favorably on its identification with perseveration. Certainly it is clear from later writings that he is careful to draw a distinction between perseveration and whatever it is that is measured by the 'p' tests. In a very thorough analysis (81) of the operations involved in these tests, he outlines four theories that may be brought forward to explain them.

The first theory is that of inertia, according to which the tests measure the hindrance effect which springs from the persistence of an aftereffect which is either neuromuscular, psychophysiological, or ideational. A second theory he calls that of the "extraneous mediation of w-characteristics." The tests, in this view, measure the sum of many character qualities which converge to influence the score; such qualities are emotional stability, self-control, inferiority feelings, and so on. The unfavorable qualities disturb the individual and prevent his making a good performance; the favorable qualities permit him to do his best. The third theory, that of "intrinsic 'will' function", assumes that the relation to will or con-

trol is direct. There are no fundamental differences in the continuance or hindrance effect; it is a matter of conscious or subconscious control or the lack of it. Every activity will continue if there is nothing to stop it; the person with poor will-control has little or nothing to stop the activity once begun, and hence a previous or an habitual activity interferes with the subsequent or different activity required of him in the tests. The fourth theory proceeds from the concept of mental set. A new task can be performed well if a pronounced mental set can be established in keeping with the task. The low scorer is one who successfully establishes a set for the new or the alternating activity; the high scorer, one who fails to establish the necessary set. If perseveration means adhering to a mental set, then the high score (according to the usual method of computation) should indicate the low perseverator; while the low score should be earned by the high perseverator.

Of these four theories, each is regarded as possible, none as adequate alone to explain all the facts. The preferred theory, in Stephenson's opinion, is that of "intrinsic 'will' function."

In another article Stephenson clarifies his own theory in regard to the 'p' factor and perseveration. Perseveration is not measured by the usual ideomotor tests. In itself it is nothing more than a minor symptom which is comparatively rare even in mental patients and of little value alone.

The difficulty and resistance experienced in the tests is an experience common to all persons when they are asked to make an effort. The one who makes least effort gets the best (i.e., the lowest) score; the one who makes the greatest effort is likely to meet with the most trouble and get the poorest score.

The truth is that the p-tests, first and foremost, present miniature life situations to the individuals who attempt them. They offer a slight difficulty to be overcome, and allow for the interplay of conation and motivation, of effort, purpose, will, in a word of character. P-tests are thus direct tests of character, and it is not necessary to consider that perseveration plays any essential part in them. (82: 50)

Cattell began with a rather sweeping theory which he subsequently pared down as the years went by. In 1933 (9) he outlined five forms of perseveration:

- 1) Perseveration of response through the referring of different ideas and stimuli to a single major sentiment or complex (delusions, melancholia, consistency of character.)
- 2) Perseveration due simply to mental asthenia or lack of spontaneity, which permits any process of thought or action once started (by external stimulation) to proceed unusually long without interruption.
- 3) Perseveration due to the very nature of the nervous tissue and analogous to the inertia of the physicist, i. e., something which shows itself as a lag in all nervous processes, resulting in some interference of all consecutive mental activities.
- 4) Perseveration as an obstinancy of old habits in the face of habits being newly formed, or vice-versa.
- 5) Repetition of old and inappropriate responses in new situations, which are really due to quite unrelated causes, usually to low 'g'. In the latter case perseveration is due to an urge to act which is not guided by any appreciation of the form of the new response required. (9: 22)

Of these five types, the last is not true perseveration. Perseveration is found in cognitive, affective, and conative ex-

periences, and there is indication that this is identical with the perseveration manifested in the ideomotor tests. Some forms of introspected perseveration, i. e., the persistence and repetitive phenomena which we consciously experience, are related to the perseveration of the tests, and some are connected with high 'w'.

It is clear from the article just referred to and from other writings (10, 11) that Cattell thought the motor tests measure perseveration and constitute a unity. It is also clear that he makes agreement with these tests the criterion for determining whether other phenomena are really perseverative or not. The high perseverator, in his terminology as indeed in that of others of the English School, means the person who earns a high score in the ideomotor tests. Various characteristics are attributed to the extremely high and the extremely low perseverator. The cataloguing of these characteristics would be rather tedious, especially since one is justified in being sceptical of the basis for such assertions. In general, perseveration is related to goodness of character in a curvilinear fashion, so that both the high and the low perseverators tend to possess several very undesirable qualities, while the medium perseverators tend to possess a stable, desirable character. The really difficult characters are found principally among the two extremes, although the defects are not precisely the same for the two groups. Considerable clin-

ical value is claimed for the 'p' tests on this basis (11, 12).*

From time to time Cattell has advanced further conclusions, hypotheses, and modifications of his position. He has suggested that high perseveration indicates deep conflict, discouragement, and inhibition which go back to frustration in early life (12). Though related to character, it is rather a matter of temperament and is associated with various emotional characteristics. High 'p' goes with disintegration of will and with certain types of maladjustment; in children the degree of adjustment seems to be reflected in the highness or lowness of the 'p' score (12). Of late (14, 15, 16) he has abandoned the theory of a single general factor for that of several factors which enter into different tests and in different proportions. The core of his original perseveration is "disposition-rigidity,"** a concept that was first proposed, though not this exact term, by Walker, Staines, and Kenna (14, 15, 89). A disposition is simply a habit, and disposition-rigidity is the relative inability quickly to set up a new habit which is similar to a given old habit. This rigidity is an "inertia of structure," not of "process" in the Spearman sense. It is a

* That the basis of these assertions in regard to character is shaky will be apparent, aside from a critical reading of the articles mentioned, when we consider the evidence brought against the tests from the standpoint of lack of unity or constancy -- to say nothing of the scoring difficulties.

** Subsequently we shall omit the quotation marks.

unitary factor, but is found only in the motor tests of the creative effort type, i. e., in tests which consist in opposing a new habit to a similar established habit and measuring the difficulty experienced in creating the new habit. Besides this, there are two other factors in the sphere of behavior once covered by his one general factor. (16).

These are the principal theories of perseveration. Naturally there are many varieties of detail among the various writers who have worked in this field. The greater number of them favored or opposed the Spearman theory in some form or other, while many have restricted themselves to a limited concept of perseveration because they were investigating a limited field. Besides the authors we shall mention later and the German workers after Mueller and Pilzecker, there are many others who have been interested in perseveration in some form yet have not had in view a theory of comparable breadth to those we have been reviewing, or have had hardly any theory at all. We have in mind, for example, those who have mentioned perseveration in connection with word association or the Rorschach (50, 51), those who have been interested solely in the clinical symptom seen in the brain-injured, the feeble-minded, in schizophrenics and in other mental patients (83). Though the general theorists have usually envisioned these other symptoms and manifestations and have at least considered the possibility of linking them with the perseverative phenomena measured by the

various tests of the English and Dutch schools, this broad outlook has usually not been found among the students of clinical perseveration, or else they were devoted to some other very different theory. Still others have used the term perseveration to designate some particular phenomenon met in their investigations. To trace these various uses of the term and the theoretical significance attached to the phenomena in question would be an almost endless task and would hardly repay the effort.

Methods*

It will simplify subsequent discussions if we describe here the various kinds of tests that have been used to measure perseveration. One large class has been designated as objective in the literature; into another and smaller class we shall put questionnaires, ratings, and observations of behavior. The last-mentioned, it will be observed when we come to describe them, have as much right to the name objective as the other tests. Among the objective tests, we may distinguish three types: 1) sensory tests, 2) motor and ideomotor tests, 3) tests of perseveration in the realm of emotions and ideas or judgments. It will not be easy to fit some of the tests into this mold because it is quite debatable what area they tap; some we shall be unable to classify exactly.

* The principal references here are Cattell (9, 13), Stephenson (81), and Spearman (77).

The sensory tests have actually been concerned mostly with some form of flicker rate or sensory adaptation; these, in view of the theory behind the work in this field, were taken as direct or relatively direct measures of the phenomenon in the field of sensation. The flicker or fusion test, both terms are now used rather indiscriminately, takes two forms. In the earliest work, papers of two complementary colors were put on the color wheel, the speed of rotation was gradually increased, and the speed determined at which a grey surface first appeared. At this point the grey is not smooth, but presents a flickering appearance. If the speed of the wheel is further increased, this flicker is replaced by a smooth grey. The speed at which the latter change occurs is called the critical fusion frequency (CFF). White and black may be used in place of the colors. An almost identical measure is obtained by reversing the process so as to start with such a high speed that the disk appears grey and then slowing the wheel down until the subject reports colors, if they are used, or flicker, if black and white are used. This point at which flicker appears has come to be called the critical flicker frequency (also CFF). Generally now the experimenter works in both directions and averages the two measures, so that the two terms have the same meaning. It need not be explained that this test is simply a convenient measure of the amount of time by which the sensation lags behind the stimulus, so as to continue after the stimulus has been re -

moved. It is a test, therefore, of inertia or perseveration as that phenomenon has been understood in the sensory field.

The second type of sensory test may be called the adaptation or, more properly sometimes, the recovery-time test. If the sense involved is vision, the subject is exposed to a bright light for a few seconds. A spot of dim light, a dimly illumined figure, or a dim image on a screen is then presented, until the subject reports seeing the light or is able to identify the figure. The time between cessation of the bright light and detecting the dim light or figure is the measure of perseveration. In the case of sound, one uses a loud and weak sound; for touch sensation, strong and weak electric currents are convenient. Aside from technical difficulties, any sense which shows adaptation and recovery can be enlisted in these tests. Instead of taking the time required to perceive a stimulus of fixed intensity, one can also determine the threshold at a given time after the application of the strong stimulus. Another rather direct measure is the duration of the negative afterimage. While the ordinary visual afterimage lends itself most readily to this purpose, any such phenomenon may be used, as, for example, the afterimage of seen movement (73).

There is a type of test which has been called sensory, though it probably involves memory and judgment as much as sensation. Working with the Weber weights, one takes a

measure of the effect of time error; he may do the same with the judgment of the intensity of sounds, and so on. In another test that is only partly sensory one uses colored papers. The subject is shown a red or blue of a given shade; a deeper shade of the same color is then exposed, and finally a group of shades among which the subject is to designate the one first shown. The measure of perseveration is the degree to which the match departs from the standard in the direction of the interpolated shade. As a control measure of the subject's memory for shades, apart from any interference, a preliminary series is run without the interpolation.

The so-called motor or, better, the ideomotor tests are legion and can be multiplied at will. The great majority of them concern the motor activity of writing. These latter are of two types: creative effort and alternating. In the creative effort test, one writes, for example, the letter S in the usual way for thirty seconds at maximum speed; he then writes the same letter backwards for thirty seconds. The short period is designed to relieve boredom and fatigue, but a larger sample for more reliable measurement is achieved by repeating these sessions. The score is obtained by adding up the number of S's written in the usual way, and the number of S's reversed. Designating the first as X, and the second as Y, one calculates $X-Y$, X/Y , or $X-Y/X$ to get a measure of the degree to which the new activity (S backwards) is hindered by the

firmly established habit of writing the S correctly. One can take letters, numbers, familiar words, phrases, symbols (like &) for the strange activity, he can write them backwards, backwards and upside down as they would appear in a mirror, with reverse stroke (so as to begin at what is usually the end, but to produce a normally appearing letter). Some have used mirror-drawing as with the Whipple Star, copying a passage of prose but abstaining from the wholesome practice of dotting the i's and crossing the t's (the I-T test), or changing all capitals to lower case and all lower case letters to capitals. The possibilities are unlimited.

In a purely alternating test, one takes two familiar activities such as writing ABCD and abcd. Two thirty-second sessions are devoted to writing ABCD over and over again; two, to writing abcd. These two together are now the X activity, and represent the subject's speed when he does one thing straightaway. Finally, four sessions will be given to writing these blocks of letters alternately, i. e., ABCD abcd ABCD abcd ... This is the Y activity. The score is X-Y, X/Y, or X-Y/X, and stands now for the interference felt in turning from one task to another in rapid succession. Again one can multiply particular tests at will. A great number of the alternating tests involve a familiar and an unfamiliar task and hence involve creative effort also; thus the "Aitches" test consists in H normal, H on its side (I), and then the two in alternation.

The scoring is as before.*

A purely motor test, in as far as that is possible, consists in taking measures of rhythmic activities. In one test, the subject taps on the table, at what he considers a natural rate, using his whole forearm and staying within an amplitude limited to six inches by an obstruction; the number of taps per second in a thirty second period is the score. This may be followed immediately by a test of the persistence of rhythm. A metronome is now set at one-half the subject's previous speed, and he is told to beat in time with it for one minute. After a set interval, S is asked to resume his natural rate. He is likely to strike a rate intermediate between his original speed and that of the metronome. The score is (original minus final speed) divided by (original minus metronome speed). Most intriguing of all is the Perseverameter Test (13). The apparatus consists of a small keyboard of two banks of three typewriter

* Later on, when we mention a specific test, we shall refer to it by the name it has acquired in the literature. For further information on these and other tests, consult R. B. Cattell (13).

The scoring is not always as simple as indicated above, but the principle is the same; to get a measure of normal speed and the loss in relation to this speed. Complications are introduced in many cases by not equalizing the number of sessions devoted to the X and Y activities, in which case differential weighting must be employed. Some workers have proposed more complicated formulae to eliminate spurious factors or to meet the needs of a particular test. The X/Y scoring is supposed to eliminate mere speed of writing, but its success is disputed. Cf. Walker, Staines, and Kenna (90).

keys, numbered from one to six. The subject can then be given two number sequences according to the scheme used in the writing tests. Automatic recording is possible with this apparatus.

Tests of perseveration of ideas take various forms. One type consists in assigning some form of directed serial association as the task, with the measure taken in terms of the number of associations. The high perseverator is assumed to be slow at association; the low perseverator, fast. If the score is merely the number of associations, perseveration is really measured in reverse. One of the stock examples of this test is to ask the subject to write down as fast as possible all the nouns, all the animals, all the words beginning with a given letter, that he can think of. The number of inkblot associations has also been used. A second type is similar to the motor tests in that it measures the interference of one process with another. The ordinary word-association technique may be used, in which case one may count as manifestations of perseveration the number of associations carried over from previous stimuli or associations. The score is the sum of these repetitions (50: 323 f.). Another method is to have the subject first respond with a rhyming word for each stimulus word in a series; one then repeats the series or some other list, asking the subject to give the first word that comes to mind. The score is the number of times he rhymes the stimulus word in the second series. Such a test can also be scored for repetitions as before.

A more complicated test consists of narratives followed by a set of questions. A brief story is read to the subject and he is immediately required to answer a set of questions about the story. A second very similar narrative is then read him and another set of questions, many of these also very similar to the previous set, is presented. The score is the number of correct answers in the first set divided by the number of correct answers in the second. A similar though still more ingenious method has also been devised to tap the higher mental processes, this time under the aspect of difficulty in settling down to mental work. The subject is to do a set of short essays in which he compares two things, as, for example, England and Ireland, the Thames and the Rhine, etc. Half of the essays are to be done in four minutes, half in forty seconds; the long and the short essays alternate. They are graded for quality; the mean grade for the long essays is divided by six (to adjust for time), and the mean for the short essays is subtracted from this to get the perseveration score (53).

Still another type of ideational test measures the loss in efficiency in switching from one mental task to another. For example, the subject may be made to do a series of multiplications of small numbers, then a series of divisions, and lastly a series in which multiplication and division alternate. The score may be expressed in the usual way or as the per cent of loss in the alternating activity. This procedure may be followed

with various sorts of arithmetical computation. It may also be adapted for use with series of colored figures which are to be named first according to figure, then according to color and then alternately. A test similar to the last but more difficult has been used even with children of low intelligence (18, 19). The subject is presented a large card on which there are several rows of colored circles, the four primaries being used in random order. In the first task, the X activity, he points to and calls out in succession all the reds and blues as he moves across the rows from left to right. In the second task, the Y activity, he reverses the names, calling the reds blue and the blues red. The session is broken up and the score computed in the usual way.

Feeling perseveration is tested by some such procedure as the following. The experimenter presents singly a group of words arranged in series of six. In each series, the first is definitely pleasant or unpleasant, while the remaining five are neutral. The score is the number of neutral words declared pleasant if the first word of the series was pleasant, or unpleasant if the first word was unpleasant.

Besides the objective tests of perseveration, we have measures of the phenomenon in terms of samples of behavior (21), of ratings by observers (3, 21), and of questionnaire scores (53). The behavior method has been used with children. They are, for example, invited to play with some material. The measure is the time they stick to that play activity. The rating method is also

most easily applicable to children, who can be observed by teachers, nurses, matrons, etc. The problem here is one of defining the various manifestations of perseveration in external behavior and insuring their clear distinction from similar phenomena. The questionnaire is like any other paper-and-pencil trait test. One assembles a list of the various symptoms considered perseverative, presents them in the form of questions, and has the subject answer in some set way or by a free description of his experience in regard to the several items. The questionnaire, which will be discussed further when we come to the description of the present experiment, taps perseveration in all the spheres of human activity.

In concluding this resume, we must make separate mention of perseveration as it occurs in the Rorschach test. It is judged to be present if a certain response, which may have been good enough the first time it was given, is repeated on successive cards without much regard for the shape of the blots to which it is applied. It is not given a numerical rating, but is rather evaluated according to the content of the response, its relationship to the nature of the blot, indication of fixed ideas, or the inability of the subject to vary his responses (51: 160 f., 344).

CHAPTER III

THE FACTOR OF PERSEVERATION: REVIEW OF THE LITERATURE

Our work in the present investigation has been stimulated primarily by Spearman's theory that perseveration is a single function or unitary factor which affects a wide field of behavior and is traceable ultimately to a basic and stable characteristic of the nervous system, the same in kind for all persons but varying in degree from person to person. To perseveration conceived in this way he has also given the name of mental inertia.

The second element of this theory, the traceability of perseveration to a characteristic of the nervous system, has rather been assumed than proved. The first element, the existence of a unitary function, has been the subject of a considerable amount of research. The reasoning behind the concentration on the one part of the theory, to the neglect on the other, seems to have been that, if the various apparent manifestations of perseveration constitute a single factor of very general influence, then this unity and generality can be explained only by postulating a corresponding characteristic of the nervous system. To put the matter in terms of inertia, if there is a general mental inertia embracing a number of mental processes and consistent in degree for a given individual, the basis of

this mental inertia must lie in some sort of physiological inertia of the nervous system.

In view, therefore, of the direction that research has taken, our review of the literature will deal primarily with the following two questions: 1) is perseveration a unitary function or is it merely a name given to many functions which vary independently? 2) if it is a unitfunction or factor of some sort, to how wide a field of human activity does it extend?

The evidence on these questions is not easy to organize in such a way that it can be presented with reasonable adequacy, yet without unnecessary repetition and cross reference. In our presentation, we shall adopt the following counsel of expediency. First we shall discuss the work of the Dutch School; then we shall take up the studies oriented after Spearman's theories and professedly directed to testing the unity of perseveration throughout the field of behavior; in the third place we shall put the studies in which only a limited field was investigated or the data of which may bear on only a limited field; fourthly we shall speak of multiple-factor studies; and lastly, we shall discuss the studies bearing on the constancy of the phenomenon. At the very end, we shall attempt a brief summary and evaluation.

Early Work - The Dutch School

The credit for suggesting the possibility of an extremely wide influence of the perseverative tendency in mental

life and the value of its study for personality theory belongs, as we have seen, to Mueller and Pilzecker, but the start in actual scientific investigation of this question was made by the members of the Dutch School: Wiersma, Heymans, and Brugmans about 1906. Their theory of temperament we have already indicated. The greater part of their work was concerned with the whole realm of temperament, its analysis, determination, and further implications. However, a part of their work bears on the question of the unity of the so-called perseverative phenomena. It was their influence, furthermore, which led to the subsequent work of the English School.

The first tests of perseveration were devised and applied by Wiersma in 1906 (92). He put 11 manics, 9 normals, and a group of 18 melancholics and paranoid schizophrenics through three tests: color fusion, time for dark adaptation, and time for recovery of sensitivity to weak electric currents. His findings agreed with the implications of his primary-secondary function theory.* The melancholics and paranoics required the most time for adaptation and recovery of sensitivity; they reported the fusion of the colors at the lowest speed. The norm-

* As the reader will no doubt recall from Chapter II, primary function is the actual conscious process; secondary function is a subconscious or unconscious continuance of neural activity after the conscious process has ceased. Secondary function and perseveration are synonymous.

als were in the middle of these measures, while the manics were at the other extreme. The evidence then would be that the sensory functions of vision and touch behave in a similar fashion in distinguishing these three groups, and hence the perseverative tendency is common to these two sensory processes; furthermore, two measures of the same sensory function seem to work together. However, the numbers are too small and the work with abnormals is open to objection.

Probably the earliest evidence bearing directly on the question of the unity of the perseverational trait is from a study of Heymans and Brugmans (35). Besides the color fusion, and dark adaptation tests, in which they followed Wiersma's procedure, they also employed the critical flicker frequency, sound adaptation, reversed letters, and pronunciation of difficult words (at the natural rate and then in time with a metronome). Many of the intercorrelations are high. Their table contains fifteen r's, of which seven are .40 or above. * The range is -.19 to .72, with only three negative; the mean is .28. At the same time, their 15 subjects were studied to determine whether they were predominantly "primary-functioning" or "secondary functioning". Only 4 were put in the first

* We shall follow the practice of indicating the sign only when the correlation is negative, omitting the plus sign with positive correlations.

class; 7 in the second class; the others could not be classified. The averages for the two classes differed in the expected direction: i.e., the "secondary-functioning" students had higher scores on the tests. That perseveration or secondary function is a distinct factor would be indicated by the fact that it is practically independent of imagination and memory (r 's were $-.02$ and $.03$), is negatively related to concentration (r was $-.18$), and is only slightly though positively related to intelligence (r was $.14$).

From these rather promising results the authors conclude that the tests measure the same thing, which can hardly be anything else than secondary function. Whatever one may think of the logic of this conclusion, he can attach to it only a limited value as suggestive of future research. The numbers were so small that a correlation of at least $.641$ (134: 212) would be required for significance at the 1 per cent level, while the difference between the "primary-functioning" and the "secondary-functioning" would be highly unreliable.

The main work of the Dutch group was a Herculean study by Heymans and Wiersma (36, 93) of the inheritance of temperament qualities. A questionnaire was sent to all the physicians of Holland with the request that they select a family well-known to them and rate the father, the mother, and at least one child on 90 traits, among which ten were concerned with secondary function. It is unfortunate for us that their

exhaustive analysis of the 2,415 records (437 families) does not include a study of the unity of secondary function; all that they have for us is the conclusion that nine of the ten traits show the influence of like-sex heredity. This would point to a common root which would be a little too vague for our purpose. Similar results were obtained from Heymans' (34) analysis of the biographies and autobiographies of 110 famous men.

Major Studies Suggesting a General, Unit Factor

In passing on to the studies which were inspired by Spearman's theories and afford evidence upon the question of a unit trait which runs through many types of behavior, it is appropriate to begin with Webb (91), since he affords us a link with the Dutch School. His classical investigation resulted in the identification of the 'w' or will factor. This factor, which he found a unity and which subsequent investigators found to consist of two or more factors (16, 30), possesses some likeness to the secondary function and perseveration of the Dutch School, and Webb in fact considered the possibility of so identifying his new factor. Their theory had made perseveration rather a virtue resembling persistence and perseverance, and it was precisely the qualities of persistence, consistency of action, and dependability that loomed large in the traits among which the 'w' factor was found. If one were to prefer the identification which Webb rejected, this would be

evidence for a unity of function for perseveration in a rather wide field, since this 'w' was a general factor running through a group of 48 traits related to emotionality, self-attitudes, sociality, activity, and intelligence. However, his refusal to identify the factor with perseveration has been generally accepted.

Webb's study was complemented by that of Lankes (53) done at about the same time. His object was the investigation of perseveration as such and its relation to character. He used a battery of tests designed to sample the various processes on which perseveration was thought to have an effect: sensory, motor, ideomotor, visual memory, higher forms of memory, and concentration; his questionnaire tapped the fields of spontaneous reproduction on the ideational level, immediate after-effect and interference in intellectual activity, conation and the formation of habits, as well as purely sensory and motor phenomena; while Webb's rating scale for character traits afforded an estimate of the "persistence qualities of character." His subjects, 47 training-college students, were the same as those employed by Webb. While the intercorrelations of the various perseveration tests, including the questionnaire, were very low, they were almost all positive; whence he concludes, and Spearman after him, that perseveration is a unitary function. The relationship to 'w' was negative ($-.40$ when corrected for attenuation); from which he concludes:

The self, with persons used to act morally, from higher motives of reason and principle, not according to merely natural bent and inclination ... can modify and directly counteract, its own nervous system and its innate tendency towards perseveration or the opposite. (53: 419, 77: 302)

It appears, therefore, that we have some evidence though it is anything but conclusive, that perseveration is a unitary trait which extends its influence through the sensory, motor, and ideational spheres, but not as such to the volitional. It is a general factor, but stops short of will activities, which it may influence only indirectly.

Another worker of the Spearman School who brought forth evidence for the claim of a unitary function was Wynn Jones. With a group of 77 children averaging about 12 years of age, he used a battery of nine tests among which four were of the ideomotor type: the I-T test, S forwards and mirrorwise, digits forwards and with reverse stroke, and, lastly, the same digits forwards and mirrorwise. The correlations among these tests were moderately low but significant, ranging from .340 to .560 and averaging .486. Analysis by means of the tetrad function revealed a single factor in these tests. That this was not mere motor dexterity was evidenced by the fact that separate tests of this purported ability, which were also employed with these subjects, had an average intercorrelation of only .086. The larger and more consistent correlations found among the perseveration tests could not reasonably be attributed to a

function showing such poor consistency.*

Later work by Jones (41, 42), however, was not entirely in harmony with these findings. He attempted a repetition of Wiersma's work with some modifications and extensions, and found that, if secondary function is measured by the color fusion and visual adaptation tests, manics show more rather than less of it than normals, while the melancholics are about the same as the normals. The two tests were not in agreement except in regard to the manics. The number of cases in each group was again small, being 8, 5, and 15 respectively. To these subjects plus a few more, he also gave ideomotor and ideational tests (I-T , digits; nouns, animals and blots). The results on the ideomotor tests are not consistent, but they do not reverse the finding just mentioned, nor do they veer toward confirming Wiersma's theory. The results of the ideational tests, on the other hand, are in line with that theory, the manics giving more associations than the normals, and the normals more than the melancholics.

* Cf. Spearman (77: 295-298) and Sen Gupta (72). Neither account gives the results of the other five tests of the original battery of nine. Although the plan of this research was reported in 1915 (40), the results were not published separately. Sen Gupta, whose description of the work appears more accurate than that of Spearman, refers to an unpublished paper by Jones.

The evidence from this research which bears directly on the question of unity of function is confusing to say the least. The correlations between tests were computed separately for the two abnormal groups, the numbers in which varied between 20 and 23 for the manics and 18 and 21 for the melancholics. As a result we have the following anomaly: the best correlations are .53, .43, and .40; but, if one looks across at the opposite column for the correlation of the same pair of tests in the other abnormal group one finds the .53 counterbalanced by -.13; the .43, by .02; the 40, by .26. The other correlations are lower and can hardly be significant when such small numbers are involved. Having before him data of this sort one is not inclined to agree with the author that mental inertia is "a factor operative in many processes."

One of the most careful bits of research in this field was executed by Bernstein (3), who administered one motor and nine ideomotor tests to 130 school children around ages 11 to 13, and had them rated for perseveration on the basis of their manner of settling down to and carrying out their school work. Actually, many of these tests were more ideational than motor. The intercorrelations among the tests were low, but mostly positive. When some of the poorer tests were eliminated the average intercorrelations were only .181 (P.E.: .081) for one group of subjects (N= 70) and .171 (P.E.: .086) for the other group (N = 60). The correlations, however, of the in-

dividual tests with the estimates of perseveration were better, four of the tests yielding the following coefficients: .390, .395, .360, and .445 for the entire group. (If the two groups are kept separate as in Bernstein's presentation, some of the figures are a little higher, the maximum being .59). When the poorer tests are again eliminated and the rest pooled, the correlation with the estimates is .48 for one group and .54 for the other. This would be a step forward in perseveration theory, since it would link the 'p' tests with behavioral perseveration, were it not for the fact that the reliability coefficients, as determined by correlating two independent estimates, were only .48 and .52 for the two groups respectively. As reliability coefficients, these are too low and leave the value of the estimates very much in doubt. Aside from this, they were of limited scope, since they were based on only one type of behavior, the ease or difficulty in settling down to a task.

If one can accept the validity of any conclusions based on low and predominantly nonsignificant correlations, Bernstein's results would indicate that the common factor in motor, ideational, and behavioral perseveration is a single rather than a "conglomerate factor", since the median tetrad difference is practically the same as its probable error (77: 305). That speed does not materially influence the score is evidenced by the fact that not one of the correlations of these tests with speed tests of 'g' was significant. A further delimitation of

the nature of this supposed 'p' factor was indicated by the fact that the subjects showed no significant diminution of score when the 'g' tests were presented in such a fashion that the subject had to alternate between one type of test and another. The switching difficulty seems not to extend to activities which are predominantly intellectual.*

Cattell has been one of the most persistent writers on perseveration, and hardly anyone has singlehanded explored more of its ramification. Much of his work, however, has succeeded in doing little more than uncover tendencies of uncertain statistical significance. His theories have, as we saw previously, undergone modification from the time of his first work in this field, and tracing them and evaluating their experimental or clinical basis would hardly repay us the effort. His earlier work pertinent to our problem we shall discuss here; the later work, which has gone into multiple-factor analysis, we shall reserve for a place among such studies at the end of this presentation.

His first important work was done in 1933 (9) as a part of an ambitious attempt to isolate the various factors of temperament by means of Spearman's methods of tetrad analysis.

* Cf. also Spearman (77: 302-306); his presentation differs a little from that of Bernstein, since he combines the two groups which Bernstein had kept separate.

His perseveration tests were the following: three of the usual 'p' or ideomotor tests, rate of tapping, persistence of rhythm (modification of tapping rate after pacing with metronome), rhyming associations, feeling perseveration (word lists to be marked pleasant or unpleasant), persistence of color image (effect of interpolated color), apperceptual perseveration, attitudes perseveration, and a questionnaire.* The subjects were a group of 62 training-college students, who also submitted to a number of other tests for the other expected factors. The correlations of the various perseverations tests among themselves were low, ranging from $-.25$ to $.23$. This evidence is no better than that from previous studies; if anything, it gives the impression of a chance distribution about a mean of zero. In discussing his results, Cattell takes the view that tests are to be judged good or bad tests of perseveration in as far as they show higher or lower correlations with the ideomotor tests. Unless he wishes to rest his case on the results of previous workers, this is begging the question. Of course, if tests correlate, they are measuring the same thing to some extent; but the fact is that the correlations are low and may be measuring nothing beyond the effect of chance factors. The items

* The test of apperceptual perseveration consists in exposing a list of words related to the theme of a previous talk on sports or teaching; most of the words are misspelled, but the subjects are to write them exactly as they are. A similar procedure is used with words unrelated to the talk.

of the perseveration questionnaire are evaluated on the basis of their relation to the pool of five of his 'p' tests, despite the fact that these are of none too certain value. It is hard to see how one can draw any conclusions about perseveration from his results, except that the existence of the trait is not confirmed. Nevertheless, Cattell seems convinced of its existence and is concerned only with the method of measuring it and its relation to other factors or conditions. He concludes, among other things, that there are indications that perseveration in the spheres of conation, attitude, and feeling is the same as that of the motor tests. He also maintains that some forms of introspected perseveration are indications of high 'w' and go with moderate 'p' score.

In a later study (10) with 52 adults, Cattell obtained somewhat better correlations. Three have the respectable values of .52, .69, and .75; but these are for different scorings of the same three tests. With these eliminated, the average correlations of his six tests are .11, .20, .22, .24, and .30. Though these are not statistically significant, they are all positive. The tetrad differences point to a common factor.

The score is the errors overlooked in the first set of words minus those overlooked in the second set. The test of attitudes perseveration consists in writing flippanant answers to ten questions, then serious answers; similarly, critical remarks are to be made to ten statements, then helpful remarks. The score is derived from the number of responses that are not in keeping with the instructions on the second presentation of the questions and statements.

With children at the ages of 10 and 14, and a battery of simplified tests, there were very poor correlations, and hence little evidence of a common factor.

Major Studies Unfavorable to a General, Unit Factor

This last finding of Cattell's brings us quite naturally to the examination of studies in which the unity of perseveration as a phenomenon of comparable generality to that of 'g' has either been challenged in the first place or has been somewhat shaken by unexpected contrary evidence. We shall begin with the work of a man who attacked the Mueller-Pilzecker concept of the perseverative tendency before the precise question of generality of function arose. Except for him, the investigators to be mentioned in the following pages have all admitted some kind of unity in some area of the ambit of behavior to which the name perseveration has been given. They denied, however, the generality of the function.

The man just alluded to was Foster (29) who published his results in 1914, about the time that the work of the English School was getting under way. With an associationist background and a rather tendentious attitude, he conducted experiments with the methods of Right Associates and Retained Members to test the genuinity of the new element. The scope of his investigation was limited to the recurrence phenomenon, which, he maintains, can be explained on the basis of association without recourse to a tendency for a former presentation to return to conscious-

ness. He says, in the course of his explanation of the introspective data, that some cases of apparent recurrence are a matter of continuance of process rather than recurrence. This admission amounts to conceding the existence of perseveration under one aspect, though denying it under another and leaves open the whole question as to the interference likely to result from something that continues in consciousness when it should presumably have departed. However, Foster does not realize the import of his admission, probably because the theories of Spearman were not yet common knowledge.

In 1931 Jasper (38) made a fairly thorough study of the double question: whether or not perseveration is a unitary function, and whether or not it affects all behavior processes. His subjects numbered 78 college students from sixteen to twenty-two years of age; 56 of them were women and 22 were men. His test battery totalled 16 tests, of which one was a questionnaire on perseveration, one was a test of introversion, another was a test of depression, while the rest were accepted 'p' tests of the motor, ideomotor, and sensory types. The intercorrelations were low practically all along the line. From the analysis of his own results and the data from Bernstein, Wynn Jones, and Hargreaves, he concludes that perseveration is neither a unitary function nor a factor in all behavior. He concedes, however, that there is some evidence of a narrow group factor of motor perseveration.

In 1935 Burri (6, 7) attacked the then current theories of perseveration in as far as they bore on her problem, which was that of individual difference in alternating tasks. Her review of work in this latter field is helpful in broadening one's outlook on the subject of alternating interference in the perseveration tests, but the studies she mentions in this connection are not concerned with the precise type of alternating difficulty found in the tests. Her own study will illustrate the difference. She had 51 college students do several tasks like addition, subtraction, drawing triangles, each task separately and continuously. The tasks were then paired so as to set two similar tasks in opposition in one series, and two dissimilar tasks in another series. The subjects were given these paired tasks under two different conditions: in the one they were to alternate every minute; in the other, every five seconds. While there were great individual differences in ability to alternate, there was little consistency for each individual from one task to the other. When the paired tasks were similar, 38 of the 51 subjects were poorer at the alternating activities. When the tasks were dissimilar, 47 were better at alternating. The frequency of alternation made little difference. Instead of one factor, she found four in her data. The first she identified as the ability to change a motor set; the second, as an ability to shift in "symbolic" processes; the other two she was not able to identify.

But there is not complete similarity between her experimental procedure and that used in the standard 'p' tests of the alternating type. The latter require alternation every second or less, while Burri's tasks were changed only every five seconds or every minute. Nor can it be said that her procedure approximates more the creative effort type of test, since the latter opposes a new activity to a well-established habit without introducing any alternating difficulty. Hence it is doubtful whether her four factors can be applied to the usual tests of perseveration.

Villamil (87) also failed to find a single or group factor in nine tests given to 102 Spanish boys from the ages of 12 to 16. However, seven of his tests, which were of the usual ideomotor type, correlated well with one another. The difficulty lay in the failure of the sentory test, which was dark adaptation, to correlate with the seven ($\rho = .056$); while the tapping test showed only a slightly higher correlation ($\rho = .122$). Unfortunately he does not give the intercorrelations of the seven tests.

Another study that we may mention here because it was expressly concerned with the question of generality of function is that of Walker, Staines, and Kenna (88). In a very clear and penetrating analysis of the principal evidence on this question, they have pointed out various flaws in Spearman's conception of mental inertia, in his treatment of the evidence, and in the

method by which the evidence was obtained. Much of their criticism is directed against the method of scoring the alternating type of test. Their contention is that to derive a valid score of interference in alternating we must first take into account the relative difficulty of the two tasks. If there is great difference in difficulty, and hence also in speed, a spurious perseveration score will be obtained, although the subject actually was just as fast and efficient at alternating as he could be expected to be in view of this difference in difficulty. They have shown, by rescoring a set of tests which had given high intercorrelations, that the evidence for a common factor of interference disappears. Since to their way of thinking, the principal test of the Spearman conception of mental inertia lies in the presence or absence of interference between temporally consecutive acts, they regard their findings as damaging to that theory.

These men have also contributed negative evidence from their own work (89,90). In one research they administered six motor tests of the alternating type to 205 university students, obtaining correlations from $-.03$ to $.24$ -- the usual level. In a comparison of different scoring methods applied to the protocols from 99 university students, they found that the size of the intercorrelations depends on the method of scoring used. The discrepancy is greatest when the two opposed tasks differ most in initial difficulty.

Notcutt (59) reports the results of the application of a battery of tests to 50 grade-school teachers. The battery was quite extensive: two measures of sensory perseveration, three of motor perseveration of the creative effort type, five of the alternating type, and four of the associative type; five tests of fluency, four measures of speed derived from some of the 'p' tests, a test of introversion, Webb's 'w' questionnaire, Cattell's surgency questionnaire, and the revised Stanford-Binet. The resultant intercorrelations are in general very small, the maximum being .31. He concludes that there is no general tendency of perseveration, but grants that there is some evidence of a general factor in alternating motor tests. The pool of the latter tests, however, correlates .41 with intelligence, a correlation which is better than that which they have among themselves.

Studies Bearing on Unity of Function in Limited Areas

There are a number of studies which, though perhaps not primarily concerned with the question of a unitary function, have contributed evidence toward the solution of this question. The evidence in some cases tends to show a unity in a very limited area, such as that of motor perseveration, or a disunity, as for example in sensory perseveration, or a distinction between two unities, one for the ideomotor tests and one for some other group of tests, and so on. The common bond between these studies is the fact that the tests employed, whether of set purpose or not, were restricted in scope and hence the

forthcoming evidence bore on only one or two areas.

The research of Hargreaves (33) on imagination appears to have sounded the first warning that perseveration might be a more complex affair than it at first seemed. His six tests of this phenomenon tapped two areas: ideational, ideomotor and motor perseveration. The two tests of the former (the naming of towns beginning with a certain letter and word-building) and one test of pure motor perseveration (tapping rate) formed one group, while three ideomotor tests (reverse stroke, inverted S, and I-T) formed another. The second group showed the usual low positive correlations among themselves (.13, .13, .30) and maintained these even when both speed and 'g' were partialled out (.12, .12, .29). Appealing to the correlations between these same tests and the ratings for behavioral perseveration found in Bernstein's work, Hargreaves concludes that only his last three tests really measured perseveration. His results, therefore, show a unity among three tests of a similar nature, which he seeks to extend to the field of external behavior. Slowness of association and slowness of natural tempo of action, which were apparently measured by his other tests, are excluded from the ambit of perseveration. Further findings are that there is no common factor linking perseveration with speed or with fluency. In regard to speed, one point is of special significance; the correlation between perseveration and this factor was not

negative, as might have been expected, but positive. Though small and not significant statistically, it would seem to indicate that perseveration is not merely the converse of speed.

Evidence by indirection is found in the work of Jersild (39), conducted about the same time as Hargreave's study. Investigating ability to shift from one mental task to another, he devised several tests in which college students were required to alternate between adding and subtracting, between adding and multiplying, between color naming and form naming, between giving opposites for certain nouns and giving verbs for them. The per cent loss for alternating as against continuous work of the same kind was calculated for the several pairs of tasks just mentioned and for the various other combinations of them. The intercorrelations of these scores are like those usually obtained for perseveration tests: for one group of 33 subjects, they averaged .21 and ranged from .07 to .50; for another group of 36 subjects, they averaged .23 and ranged from .02 to .37. Three intelligence tests (Otis, Thorndike, and Army Alpha) showed correlations of $-.06$ to $.47$ with the various per cent loss scores. Jersild's conclusion is that there is evidence for an ability to shift from one task to another, but that it is positively related to general ability. Since ability to shift implies the relative absence of the interference phenomenon attributed to perseveration, his results serve to indicate a unity in the field of difficulty of shift-

ing. They also indicate that this supposed aspect of perseveration is perhaps just an effect of relatively lower intelligence. It must also be noted that the tests, while they involved a motor function, were predominantly mental. This would easily reconcile the connection with intelligence found here with lack of connection found by some other workers who used the standard 'p' tests, in which motor dexterity may play a large part.

What we called behavioral perseveration has fortunately had some specific attention over and above the almost casual sampling of it in Bernstein's work. A very interesting and very thorough study we owe to Cushing (21), who investigated the play behavior of children. Her inspiration was derived from the Mueller-Pilzecker concept of the perseverative tendency, which was defined for the purposes of her research as:

the tendency of an individual to continue in a given mode of behavior when external pressure for continuance has been reduced to a minimum. The conditions implied in such a definition are the absence of an ultimate goal set either by the examiner, or by the nature of the material itself. The remaining drive would be presumably indicative of a certain internal momentum alone. (21: 6)

Several sets of play material were offered to 49 children in university-conducted nursery schools and 21 in a settlement nursery school. The age range was two to five years. The score was the time spent at each play activity. One of the activities, which consisted in trying to open a padlock with a "doctored" key, was used rather as a measure of persistence;

MA's and IQ's were also available.

She concludes to a common factor in her play tests of perseveration, presenting a table of correlations somewhat better than those previously mentioned. The poorest is .18 (P.E.: .08); the best, .61 (P. E.: .05); the average is .42. As a validating criterion, she devised a questionnaire of 47 items to be filled out by two teachers and one parent. This questionnaire, which had the odd-even reliability coefficient of .89, correlated .40 with the pool of the tests. Factor analysis revealed a common factor other than 'g' or chronological age, and also some group factors. The actual correlation of the pooled tests with mental age was .28 when chronological age was partialled out. It is possible, therefore, that the perseveration scores are appreciably affected by degree of mental development.

To relate Cushing's work to that of the English School, we have in her data evidence for a unity of function embracing the tendency to continue in an activity once begun. This holds both for standardized play activity and for the child's general activity as observed by and large by teachers and parent. Obviously it would be impossible to study the relation of this tendency to motor perseveration as measured by the paper-and-pencil tests; but other tests could be devised. Sensory perseveration could also be measured with a little ingenuity. It is unfortunate that these promising results have not been fol-

lowed up with children of this age.

Behavioral perseveration was also investigated by Sen Gupta (43), who conceived it as the criterion against which the ideational and ideomotor tests might be validated. Estimates of the degree of perseveration manifested in school behavior by 56 school girls were obtained by means of a ten-item rating scale; the other tests were of the usual type. Despite the author's conclusions to the effect that the objective tests measure a single factor and that this is to be identified with perseveration because of the positive correlations between the tests and the estimates, we can see in his data only a failure to substantiate the evidence previously brought forward by Bernstein. Although the two independent ratings which Sen Gupta was able to get on each girl were correlated to the extent of .721 and the reliability coefficients for the single tests were on the average even better, the highest correlation of any single test with the pooled estimates was .20 (P.E.: 085). This correlation is certainly not sufficient evidence of a connection between the 'p' tests and behavioral perseveration.

The correlations among the 'p' tests were of the usual order, though a few were fairly respectable (.352, .380, .426, and .595) after 'g' had been partialled out. These were, however, obtained mostly among the ideational tests, which Hargreaves (33) had found unconnected with the ideomotor tests.

Bernstein and Sen Gupta had attempted to show a relationship between the ideomotor tests and behavioral perseveration, thus hoping to validate the claim that the tests measure perseveration. Stephenson, on the contrary, vigorously opposes this claim. He concedes the existence of a general 'p' factor as an experimentally proven fact (80), but denies that it is identified with perseveration. Although he does not give any actual data to substantiate his claim, he argues as follows (82). The usual motor tests can be so modified as to make it impossible for perseveration to influence the performance. If, then, the modified tests are scored in the usual way they will show the usual negative and curvilinear correlation with 'w' and the usual positive correlation with 'f'. Because these same relationships are obtained whether perseveration is excluded or not, these tests do not measure perseveration.*

Pinard states that perseveration constitutes "a general factor of the mind". (62: 124) This conclusion can hardly be based on his data, since they concern only four motor tests. Nevertheless, it is true that the correlations obtained with a group of 194 institutionalized children are surprisingly consistent, though low. The minimum is .29, while the maximum is

* In explaining his method of excluding perseveration, Stephenson uses the cancellation test as an example. The alternating part is eliminated. In the remaining two parts, the letters to be cancelled in the second part are dissimilar to those cancelled in the first part. Presumably the difference of the two tasks prevents any perseverative influence of

.37; all are significant. Aside from the fewness of the tests, another objection has been brought against his work: i.e., that the scoring method (X-Y) made the tests dependent to a great extent on speed of writing.

It was in this research (61) that Pinard discovered that the problem children in a custodial institution tended to be either very high or very low perseverators, while intermediate scores were found mostly among the better behaved and adjusted. It thus appeared that the negative relationship to favorable character qualities unearthed by Lankes was really curvilinear. In a study (62) of adult patients in a mental hospital, he found a negative correlation of introversion with perseveration. Some of the items of his rating scale for introversion concerned the change or persistence of emotional states. Many of these items appeared so related to perseveration that those scoring high on the 'p' tests tended to experience more persistent emotions. This would be an indication of a functional unity between motor and emotional perseveration, were it not for the objection against the scoring method.

Another member of the London School to declare for unity is Rangachar (64). His evidence, however, is inconclusive. His battery of seven motor tests showed the average correlation at about the usual value: .287. This might have been taken as a further token of consistency with other findings,

the first on the second (82).

had it not been for the author's careful analysis of his results. Upon observing that some subjects wrote faster than others in the first part, or X activity, of the tests and that the scores correlated appreciably with speed (.08 to .88), he employed weights for the several tests so as to eliminate the speed factor. As a result the correlations fell considerably, the maximum now being .19. Nevertheless, he found a common factor. In criticism of this last conclusion, we may say that his maximum correlation could not be significant (N was 73), and that it is hard to see how one can fail to get a low tetrad difference when all the correlations are low (.07 to .19). If all the correlations are close to zero, they will remain close to zero, no matter how they are multiplied by one another.

Other evidence that, like the rest on this point, just falls short of being satisfactory is found in the interesting study by K. H. Rogers (67, 68). With one group of 220 school children, he obtained intertest correlations of .23 to .32, which were all positive and at least five times their probable error (.04). It is unfortunate, however, that the value of these results is reduced to almost nothing by the fact that only three of the usual motor tests were involved. With a group of 34 children of subnormal intelligence (IQ: 50 to 70), the correlations of a battery of six tests, carefully chosen and administered individually, were as low as usual: -.09 to .33. Nevertheless, the tetrad analysis led Rogers to

conclude to a common factor. But we are up against the usual problem of the statistical significance of the original correlations. Rogers also concluded that this common factor is different from 'g' because the tetrad differences when the latter is involved are almost significant. This is hardly convincing.

In still another study (69), he used two motor tests (but four measures) of shifting difficulty with 75 college students. Correlations were again low: $-.27$, $-.03$, and $.24$. His conclusion this time was that there was no evidence of a common factor. One aspect, however, of his procedure deserves further investigation and may possibly prove more fruitful than the statistical methods. He took introspections. On the one test 26.5 per cent of his subjects, and on the other 31.8 per cent, reported a conscious experience of interference when asked to switch to the second of two similar tasks. Analysis of introspective evidence could hardly prove less disappointing than the statistical results hitherto, and might lead us to the phenomena really at the basis of the perseveration tests.

Downey (25) professes to measure volitional perseveration in her Will-Temperament Test by means of the time that the subject spends in the disguised handwriting subtest. She mentions various correlations with various other traits measured by her test, but gives no evidence that there is such a thing as volitional perseveration beyond showing that the score here used shows an appreciable discrepancy from the self-rating for

perseverance given by 21 subjects. This is poor evidence of a distinct trait. Though the work of Lankes and some others is cited, no experimental evidence is presented for relating her measure to the usual measures of perseveration.

That there is no factor of "volitional perseveration" as conceived by Downey has been shown by Dorcus (24), who obtained predominantly low and non significant correlations between four tests of a nature similar to the one mentioned above. His tests, like Downey's, seem rather to be measures of persistence; his results agree with those of laboratory studies of this quality, which have failed to uncover a unitary trait.* It is at least extremely doubtful that perseveration may be extended to volition.

One of the most lengthy studies in the field of perseveration was that of Shevach (73, 74). Limiting himself to sensory perseveration and restricting the concept so as to embrace only the continuance, or lag, of the sensation after cessation of the stimulus, together with the consequent interference with subsequent sensations, he put to himself two questions:

- 1) Is sensory perseveration an innate and fixed characteristic

* Cf., for example, Kremer (131) and Thornton (138, 139). The weight of the evidence seems to be on the side of several rather independent factors entering into the tests of persistence. Even Webb's 'w' has shared this fate. Cf. Garnett (30).

of the nervous system which affects all sensory processes, or does it depend on the sensation involved? 2) Is it constant under all conditions. A third question we have omitted as not pertinent to the present discussion.

The tests included adaptation of light, sound, thermal and tensive stimuli, the limen of sensitivity to an electric current, recovery of sensitivity after electrical stimulation, negative afterimages and aftermovement, the time error in lifted weights and with the sound pendulum, and finally the number of oscillations per minute with the illusory cubes. He thus applied the adaptation or recovery measure to every sense that could conveniently be studied in this way, while his last three tests brought him over into the field of perception. His subjects included 12 children from nine to thirteen years old, a group of 12 undergraduates who had scored low in personality tests, another group of 13 undergraduates, 11 graduate students, 13 personal friends ranging in age from twenty to forty, and 17 unemployed persons. Treating each group separately and varying the procedure somewhat as he attempted to investigate various factors, he presents data which paint a picture of confusing and contradictory results.

As to our principal concern here, the intercorrelations of the perseveration tests were low and contradictory from one group to another. For example, some of the average intercorrelations of the tests were: for the unemployed, .195;

for the experimenter's friends, .012, for "neurotic" college students, -.015. In one of the groups, the variability in perseveration score from one type of test to another appeared to behave in a unitary fashion, in as far as it correlated appreciably with two measures of neuroticism (supposing that these measure the same thing), the rank-difference coefficients having the value of .32 and .57 respectively. With another group, however, the correlations were lower and failed to have significance. The latter group then repeated the tests after being told that their previous performance had been subject to inaccuracies and peculiarities on their part. Their variability scores on the two sets of tests showed a zero correlation. The intercorrelations of the perseveration tests were also affected, their average changing from -.015 to .13. Hence, neither sensory perseveration nor variability showed any real consistency.

Because of the divergent results with different groups, Shevach concludes that sensory perseveration shows a functional unity in some individuals, but not in others. This means, concretely, that some show the same degree of lag or afterfunction in all sense modalities and with various types of test, while others do not -- a conclusion that would be tantamount to the position that the several sensory processes do not in and of themselves possess the same fixed degree of lag, but may, for some reason or other, have a unity imposed on them in a given individual. This evidence, therefore, if accepted as reliable

would tend to show that sensory perseveration is not a constant factor in the sense in which Spearman and others meant it: namely, a factor found in all individuals. However, it is risky to base any such conclusion on Shevach's data, since the samples were very small. We cannot be sure that the divergent results were not merely due to the chance fluctuations to which small samples are extremely prone.

Later results obtained with the same tests by Shevach (75) display the same low correlations with groups of 18 adult Jews and 19 adult Gentiles. However, with 12 Jewish children, the intercorrelations among six of the tests are surprisingly good: .17 to .87. The author concludes to a functional unity with children, a rather justified conclusion except for the old difficulty that the numbers are too small to admit of significance for any but the very highest correlations.

The findings of Rabin (63) are pertinent to the question of sensory perseveration and its relationship to the ideomotor tests. He employed two tests of the former and two of the latter with two groups of mental patients numbering 32 and 80 respectively. The correlations obtained with both groups agreed in indicating no relation between sensory and motor perseveration; they were not in agreement in regard to the relationship between the tests of the same type.

A further point in regard to sensory perseveration is brought up by Clarke's (17) results with the flicker-rate test.

she found that this purported measure of sensory perseveration did not correlate very well with a battery of five ideomotor tests. There were two measures of CFF. While the correlation between these two (.723) was sufficiently good to show internal consistency for this test, each yielded a poor average correlation with the ideomotor tests (.128; P. E.: .094 and .148; P. E.: .094 respectively). Because of this she concluded that flicker-rate is not a good test of perseveration. In regard to the ideomotor tests themselves, the average intercorrelations ranged from .220 (P. E.: .048) to .334 (P. E.: .045) for her entire group of 182 boys and girls.

These results unfavorable to CFF were not accepted by Biesheuvel (5) at their face value. Contending that the method of measurement had been faulty, he introduced an appreciable refinement of procedure and obtained a large number of readings of flicker-rate from two groups, 19 and 23 boys respectively. He also administered a perseveration questionnaire. Treating the data of each group separately, he divided the perseverators from the nonperseverators in each group, on the basis of the questionnaire. The difference in mean flicker-rate between the two extremes in the one original group was 2.2 cycles (S.E.: .84); in the other, 1.5 (S. E.: .386). These differences, he concluded, are statistically significant and indicate that CFF does measure perseveration. However, it is to be noted that the number of children in each subgroup must have been small, ap-

proximately half the original number, which was 19 in the one case and 23 in the other. Numbers as small as these would call for the use of Student's t or some other statistic designed for small samples. Since the more accurate criterion was not employed, we may well hesitate to accept the differences as significant. The whole matter of the relation of CFF to perseveration remains obscure.

To return, then, to the ideomotor tests, we come upon some rather curious results obtained by Hamilton (88). She worked with two groups of children equated for intelligence, but differing in educational achievement. The one group (of 75) was scholastically retarded, while the other (of 50) was normal. There were five tests of the alternating type. The intercorrelations were considerably different for the two groups; for the normals, they ranged from $-.14$ to $.25$; while with the retarded group they were all between $.55$ and $.78$. Tetrad analysis gave clear evidence of a common factor in the latter case. This would invite the conclusion that perseveration in the form of shifting difficulty manifests a functional unity with the scholastically retarded. Unfortunately, however, this evidence is cast in doubt by the work of Walker, Staines, and Kenna (88), to which we have previously referred. Upon rescoring Hamilton's test protocols with a special method designed to eliminate the factors of speed and the initial difficulty of the two tasks more adequately, they found that the correlations for the re-

tarded group ranged from $-.19$ to $.37$. Thus the figures were reduced to practically the same level as those for the normal children ($-.20$ to $.21$). If one were to accept Hamilton's original correlations, the unity of function would apparently be restricted to children who are retarded only scholastically, since with a group of 15 mentally retarded children (IQ: 50 to 78) Collins (18, 19) obtained correlations of $-.42$ to $.49$, with the average at $-.26$.

Collins' other data are of interest too. While she found the low correlations previously mentioned for her battery of tests with a group of low intelligence, those for her average and above-average groups were rather good. For the group of average intelligence, $.21$ was the lowest and $.58$, the highest correlation, while the mean was $.41$. For the superior group, one correlation was $-.12$, while the others ranged from $.20$ to $.82$, with the mean at $.45$. These correlations, however, are of reduced value because the number of subjects in the two groups was only 15 and 16 respectively.

At this point we may refer again to Walker, Staines, and Kenna (88, 89). They concede that there is a narrow group factor to be found in tests of the creative effort type and also in those alternating tests which also involve creative effort. In regard to the latter, it is precisely this element of creative effort which accounts for the intercorrelations often obtained with these tests. The factor of interference

in alternating is an artifact of an inadequate scoring method. This narrow factor is not mental inertia in Spearman's sense, since it does not involve the effect of a preceding on an immediately subsequent act, but rather of a fixed habit on a new activity with similar components. For this factor they suggest the name: habit rigidity or habit inertia.

After experiments in which he seems to have taken the perseveration tests at their face value (26, 27), Eysenck declares against the generality of this function in his recent book on personality factors (28). Here he reports the work of one of his associates, A. Petrie, who obtained poor correlations between four of the usual motor tests; only one of six correlations was significant and positive, while one was significant and negative. He regards this as confirming the findings of Jasper, Burri, and others.

Multiple-Factor Studies

The outcome of the many studies, while they leave the perseveration theory very much confused, has been to emphasize one thing. Despite the tenuousness of the evidence in many cases, there is a unity of some sort in some of the alleged tests of perseveration and with some groups. With a situation like this, the possibility of many factors is suggested. Accordingly we shall next consider the results of research in which perseveration measures were a part of a general multiple-factor study.

The first of such studies was that of Line, Griffin, and Anderson (54,55). Their object was the discovery of the factors making for mental health; tests of perseveration were simply one set of objective tests employed. Since they do not report the intercorrelations of their five 'p' tests, we can not compare their results with those of other studies from this standpoint. Of very great interest and significance, however, is their isolation of two factors running through all the tests used. The first and principal one is named "objectivity" and is tentatively regarded as making for mental stability. The other is probably a group factor, and is termed "fluency" or "mobility". The perseveration tests had a loading of .54 for Factor I and .07 for Factor II. Hence it appears possible that, regardless of other results from studies of perseveration alone, the various 'p' tests may tap a single function broader than the usual conception of perseveration. The authors suggest that their Factor I may possibly be Spearman's 'g'. If this is true, then the 'p' tests are to a large extent measures of 'g'. If we check this inference against the published data on the relation of perseveration to 'g' or intelligence quotient, we find some that would fit in with this hypothesis in as far as some of the correlations of 'g' and 'p' are about as good as those between the 'p' tests themselves. We should also find contrary evidence.*

* To choose two examples of opposite results, Coll-

In a trait study extensive for the number of subjects involved, 645 children in Scotland and South Africa, though not for the number of traits investigated (only 31); Biesheuvel (4) revived the typology of Wiersma, Heymans, and Brugmans, interpreting perseveration in terms of secondary function and making the primary-secondary function continuum one of the three basic dimensions of temperament. He finds it, after factor analysis, a basic behavior unit. His conclusions, however, will not detain us further because they are not related to the perseveration tests employed by other workers and appear based on too few traits in the first place. One may well ask what is the value of a study which purports to cover the whole field of temperament by means of an assessment containing only 31 items. It may be noted in passing that Biesheuvel revives the notion that perseveration makes for desirable social behavior.

Another factorial study of rather limited value because of the limited scope of the tests and ratings was that of Reyburn and Taylor (66). They used only three tests of perseveration derived from Cattell and ratings of ten items in an

ins (18, 19) reports a correlation of $-.693$; Notcutt (59), of $.41$, for tests of the alternating type. All of Collins perseveration tests were of this type. Both used the Stanford-Binet for obtaining the IQ. These are about the highest correlations reported at either extreme (minus or plus). Several have reported near zero coefficients: for example, Cattell (10), Rogers (67).

introversion-extraversion scale. The correlations were, as usual, low. Factor analysis yielded four factors, of which one appears to be perseveration. Its loading in the 'p' tests is .48. Oddly enough two items in the rating scale also have a significant loading: 1) "is careful of the feelings of others," and 2) "is easily hurt." It will be noted that the second item obviously involves emotional behavior, while the first probably does so also. This connection, a very tenuous one, may be an indication that these very narrow 'p' tests do have broader roots in the personality sphere.

Evidence of a factor probably identifiable as perseveration is also forthcoming from the study of Dorothy Rethling-shafer (65). With a battery of tests yielding fifteen measures of persistence, nine of perseveration, two of continuance of interrupted activities, the Otis Self-Administering Scale, and two attitude tests, she obtained seven factors. Two of these are pertinent to perseveration. The first, which is identified as the habit of finishing what one starts, had an appreciable loading in two 'p' tests from Cattell and in one other reputed test of perseveration. The second factor, which is tentatively identified as perseveration, was found in several tests which have often been used to measure this quality, but not in sensory-perseveration tests or the classical 'p' tests. The tests having appreciable loadings with this factor are: tapping (both natural rate and effect of imposed rate), jumbled letters (both

time and number of words), length of maintained grip, and questionnaire items indicating the recurrence of ideas, dreams, etc. There was also the suspicion of some obscure common factors in the tests of sensory and motor perseveration. That the 'p' tests are thus related to a character trait, finishing what one starts, is very much in line with Stephenson's idea of what these tests measure.*

Cattell, in his later work on perseveration, used the methods of multiple factor analysis. In one study (14, 15) in which the interest correlations were as low as usual save for an r of .60 between two very similar tests, he discovered two factors: the first of which is labelled "disposition-rigidity" or the difficulty of creating a new habit which is similar to an old, established habit, and a second, smaller factor of doubtful meaning. Disposition-rigidity is, according to him, a unitary factor, but is found only in the creative effort tests. The doubt in regard to the meaning of the second factor was apparently removed before the publication of his book on personality description and measurement in the same year (16: 433-442). Here he reports three factors: disposition-rigidity, mental versus motor perseveration, and clinical perseveration or Ego rigidity. The second is manifested in the alternating tests involving subtraction, addition, giving adjectives and verbs for nouns in an association test. The third is present

* Cf. Chapter II, pp. 17 f.

as a positive factor in tests of the effect of an imposed rhythm on the natural rate of tapping, in the time needed for the solution of jumbled-letter tasks, in maintained grip, in natural tapping rate; it is present negatively in the absence of recurrence effects and in the number of words one succeeds in making out of a certain number of letters. This third factor does not correlate with the first. These results were obtained from his extensive work on the identification of the basic factors of personality, which work the present author is not competent to criticize. Cattell himself admits, however, that the perseveration tests and the published findings do not satisfy the usual statistical criteria. To this self-criticism we may add the much earlier observation of T. L. Kelley (44: 20 f.) that low correlations with high probable errors are not adequate to prove a factor of perseveration. Since the correlations in the subsequent work, upon which Cattell drew in his factor analysis, have not materially improved, this criticism seems still valid. Kelley, it must be remarked for the sake of accuracy, does admit that the data suggest the presence of other factors besides 'g', although they cannot be identified.

Constancy of Perseveration Test Scores

The review of the evidence for and against the theory of a unitary function of perseveration would be incomplete without a report of the work on the constancy of test scores or

perseverative phenomena from test to test or from day to day. We can also profitably review evidence of one or two variable factors which may affect one's test score. In concluding this section, we shall return for a moment to the question of different methods of scoring.

In order to render the extensive work on the supposed perseveration factor of any genuine value, it is necessary to show that the scores on the tests or ratings are sufficiently constant when disturbing factors have been eliminated. If it turns out that the perseveration score of a given person varies considerably from day to day or from test to retest, and these changes cannot be accounted for by any known variable factors, then the value of measuring a person's perseveration is decidedly questionable, even though in a given test situation it should act like a unitary trait. It would be doubtful that the unitary trait was more than a momentary phenomenon, perhaps a reflection of a passing subjective condition.

Since practice effect is one of the commonest causes of changes in test scores, we may conveniently start with the evidence brought forward by Culler (20) as early as 1912. Though not directly pertinent to perseveration, it nevertheless bears on our question. Culler was concerned with the process of establishing two conflicting habits at the same time. Writing two lists of numbers on the typewriter and sorting cards in two conflicting sets of categories were his principal experimental

methods. He found that all subjects improved with practice and that interference was only an incident in the course of making the opposing activities automatic. Since his tasks were similar to those used in the alternating tests of perseveration, these results would provide a warning that one must insure either that his subjects were all entirely unpracticed or all equally practiced before a valid measure of switching difficulty could be obtained. A further warning lies in the finding that there were individual differences in the rate of improvement. This would suggest the likelihood of variations in relative score on successive tests.

The first thorough investigation of the constancy of the perseveration score with repeated tests was that of Cameron and Caunt (8). They had 10 normals and 40 abnormals take the same test ten times, at six-minute intervals, in the course of an hour. Aside from other aspects of their procedure and from other conclusions, they found that the score fluctuated considerably from one test session to another during the course of the hour. The means of the whole group for the ten test sessions varied from about 6.25 to about 14.25. Their conclusion was that it is of little value to estimate perseveration by means of a few tests which are completed in a few minutes, but that a series of tests carrying over some time might be of more value. This last is apparently based on their finding that, if the score is high in the beginning, it tends to remain high for

some time.

Cattell (10) also noted a practice effect when he re-tested 50 children of 10 years of age. The average score in the first testing was 5.61; for the second testing, 5.11, a change of about 30 percentile ranks according to the norms for his tests. This change was most marked with tests in which one activity was new, as in writing letters with reversed strokes. He also found a fatigue effect. Three of the children were tested four times in six days at 9:30 A. M., 11:30 A. M., and 4:10 P. M. When the practice effect had been eliminated (in a rather a priori fashion), the scores of the three children averaged, and the averages plotted for each day, the result was four straight-line graphs ascending from the 9:30 point to the 4:10 point. The change would again be about 30 percentile ranks.

Some rather adverse results were obtained by Yule (94) when she determined the split-half and the test-retest reliability of eight tests. The split-half coefficients for the single tests ranged from .403 to .666, figures which are very poor for this type of coefficient. For test and retest seven days later, they were .016 to .523; while the coefficients for the pooled scores of the entire battery were .555 and .791 for two methods of pooling their results. The lower was obtained with the usual method which is to average the final scores from each test; the higher was obtained by adding the part scores and then dividing the total X score by the total Y score. In

view of the possibility that this latter method may be spurious, it appears safer to take the lower coefficient, which is certainly too poor for a battery of objective tests. It appears, therefore, that the reliability of the tests used, which are from Stephenson and are among the best of their type, is not by any means assured.

The work of Kendig (46) is pertinent here in as far as it indicates variable factors which may influence any score derived from questionnaire estimates of introspected perseveration. She investigated the conditions determining the compulsive-like return of ideas to consciousness. With very ingenious methods she had 90 subjects give a number of words that began with c; she left 30 of her subjects under the impression that the task was merely a casual affair, while she let the other 60 understand that it was a serious experiment. Of these 60, half were given to understand that they had succeeded at the assigned task; half were led to believe that they had failed. Each of these groups of 30 was further broken down so as to have one third (i.e., 10) immediately proceed to another task, one third remain unoccupied for five minutes, and one third remain unoccupied for a half-hour. Two weeks later the subjects answered a set of questions on the subsequent recurrence of c words. This compulsive recurrence phenomenon was increased if the task was originally taken seriously, and especially if there was a sense of failure. The latter effect

was itself increased if the immediately ensuing period was devoid of activity and short (five minutes). In a subsequent experiment (48) it was found that the effect of failure is greater if it is last in reference to a successful task.

Darroch's study (22, 23) of the variation of perseveration score for the same test over 50 more or less consecutive days is probably the most damaging of all. Though suitable statistical procedures showed that there were significant differences between the mean scores of individuals for the whole of a long series of repeated tests and even for samples of five trials taken at the beginning and the middle of the series, still the variation in the rank orders of the subjects from the earlier to the later trials was considerable. This was especially true of eight of her subjects (N was 16) who had not had any previous experience with the test employed.

A last study points to the possibility of some effect of previous training on behavior that appears perseverative. Restricting the play activities of one of two identical twins of three and one-half years while the other was allowed free scope, Thompson (84) found that such a restrictive regimen influenced the child to persist in one type of activity. The same results were obtained when the twins changed roles in a control experiment. The effect of the training endured for some time, but was apparently wiped out completely by six months of kindergarten. These results seem to be a warning in

interpreting the data from perseveration tests such as Cushing's, which was used in this experiment, in terms of innate temperament. They would not, however, militate precisely against the unity of perseveration, since there would be nothing unusual to finding its roots in training. One observation of Thompson's must be mentioned as strengthening the theory of an innate disposition. She noted that even when the range of the children's activities had been limited, they preserved fundamentally the same variability of attention as before; they kept at the same thing but varied their way of playing.

Related to the question of the constancy of perseveration is that of the proper scoring method. We have already seen the effect of a change of scoring on the intertest correlations, thanks to the work of Walker et al. (88, 90). Similar evidence may be found in Howard's thorough study (37). An appreciable drop in correlations was also reported by Rangachar (64) when the effect of speed of writing was eliminated by the method of partial correlation. The speed factor, however, does not seriously affect any but the crudest method of scoring: X-Y (37); so that much of the past work is unobjectionable from this standpoint. However, the alternating tests are open to criticism because none of the usual methods of scoring takes account of the difference in difficulty between the two tasks to be done in alternation. Hence very little of the evidence for a unitary function, when these tests are involved, can be accepted without

reexamination. This would be possible only if the raw data are still available.

Summary and Evaluation

One thing is rather clear from the preceding review of the literature. The great majority of the men who have published research work on perseveration are agreed that there is a unity of some kind somewhere among the various phenomena that have been called by this name. Though much, if not most, of the evidence is not very strong, we may take it as at least probable that some of the so-called manifestations of perseveration are interconnected in some way.

The case for a general factor or trait running through every sphere of human activity is rather tenuous. The principal evidence for such a general trait is found in the work of Lankes and the earlier research of Cattell. We have seen that their tests covered the various processes with reasonable adequacy and that their results were suggestive of some unity, though hardly conclusive. Whether other evidence, such as that of Jones, and Bernstein, can be taken as suggesting a general factor depends on how one interprets the significance of their tests. Their motor tests of perseveration correlated fairly well with one another, so that we have an indication that they measure the same thing; but the question of what they measure has never been answered satisfactorily. It seems to the present writer that the paper-and-pencil tests used by

Bernstein and Jones call for sense perception, motor activity, imaginal activity, volition, and a greater or less degree of intellectual activity; but that they measure, according to the scoring, an interference phenomenon that affects only the motor activity, the intellectual activity, and perhaps the imaginal. According to this viewpoint the evidence from these tests cannot be accepted as pointing to such a general factor as these two workers seem to have in mind. Bernstein's evidence would, it is true, allow him to extend this factor to behavioral perseveration, but there is the contradictory evidence of Sen Gupta. According also to Bernstein's results, purely motor perseveration would also be included, since tapping rate scores correlated with the rest. Jones' results would suggest extension of the common factor to the purely ideational sphere, and to the sensory sphere for one group. The evidence, however, for all these extensions is doubtful and contradicted by other results. The work of Biesheuvel might be adduced here, since he claimed to find a functional unity for secondary-function or perseveration. It does not, however, appear that his sampling of the various processes was sufficient.

The evidence against a general factor is of three kinds: 1) data in which a reasonably adequate sampling of the various processes revealed no evidence of a common factor for all of them; 2) data in which a number of accepted tests of perseveration, though limited in scope and hence having the

better chance of manifesting a unity, either failed to show the expected unity or on the contrary seemed to form two or more discrete units; and 3) evidence derived from lack of constancy of the test scores.

In the first category belong the studies of Jasper and Notcutt, who found none but a limited unity. We shall also mention those of Burri and Villamil who professed to be testing the hypothesis of a general factor, though, like Bernstein and Jones, they actually do not seem to have done so. Burri found one factor for motor activities and a different one for "symbolic" activities; her results are not strictly comparable because her tests differed considerably from the 'p' tests. Villamil's tests were inadequate in scope; he did, however, find that one purely motor test and one sensory test failed to correlate with the standard ideomotor tests.

In the second category belong the works of Hargreaves, Rogers (one study), Shevach, Clarke, Hamilton, Walker, et al., and Petrie and Eysenck. Though Hargreaves concluded to a common factor, his results at least show that tests of rapidity of association and tapping rate form a group distinct from the paper-and-pencil tests. Rogers found no evidence of a common factor in two tests of shift interference in motor activity, which, however, differed from the usual motor 'p' tests. Shevach found that an extensive battery of sensory perseveration tests showed some evidence of unity with some groups of adults, but not with others, and that instructions calculated

to modify the attitude of the subject could materially change the scores. Clarke found one sensory test, CFF, out of line with the results of the motor tests. Hamilton found no evidence of unity for tests of the alternating type with a group of scholastically normal children, very good evidence with scholastically retarded children; Walker, Staines, and Kenna, however, showed that the latter evidence disappeared if the scoring method was changed. Petrie and Eysenck found no unity in four motor tests of the usual type.

The third class of evidence throws doubt on the basic supposition of all these tests: that something constant is being tested. We have here the findings of Cameron and Caunt, Yule, and Darroch. The first two authors found considerable variation in score when the very same test was repeated ten times in the course of an hour. Yule obtained split-half reliability coefficients which were very low as such coefficients go, while test-retest reliability was also very poor for single tests and for the pool of the battery, except when the latter was computed by a doubtfully valid method. Darroch showed that a person's perseveration score will vary considerably over a period of some fifty days. The data of Culler, Cattell, Shevach, Ken-dig, and Thompson in regard to the effect of practice, fatigue, change of attitude, determinants of ideational perseveration, and the effect of training on perseveration in play represent disturbing factors which may not have been controlled in some of

the previous work, but which could be controlled as well with these tests as with any other. The results of Walker, Staines, and Kenna's work on scoring method affect only the alternating type of test; however, since much of the evidence for a common factor was obtained with these tests, a reexamination of the original protocols of much of the work reported in the literature would be called for -- or else a fresh start.

Since the matter of a general functional unity is very much in doubt, it will be profitable to consider the upshot of the evidence of what processes do appear to go together. There is rather a congeries of evidence, though much of it is doubtful, that the so-called motor tests, i.e., the paper-and-pencil tests described systematically by Cattell and Stephenson, form at least a loose sort of unity. Aside from the results of Lankes and Cattell (earlier studies), the following may be listed as producing some evidence for this opinion: Heymans and Brugmans, Bernstein, Wynn Jones, Hargreaves, Jasper, Pinard, Villamil, Rogers (earlier work), Notcutt, and Collins. Cattell in his later work and Walker, Staines, and Kenna admit a unity only for tests of the creative effort type, while some of the evidence from other studies mentioned above must be limited to one type of test or the other merely because the workers employed only one type of test. That this unity may be extended to include other tests of motor perseveration (such as tapping tests) seems indicated by the work of Bernstein, Cattell, Burri,

and several others but is contraindicated by that of Hargreaves, Villamil and Jasper. The extension of this same unity to the sensory field is, to say the least, doubtful in view of the contrary results of Jasper and Shevach, and the inconclusive results of Wynn Jones. Clarke's and Rabin's evidence is also contrary, but hardly adequate. That there is a unity in the field of ideational and higher mental processes seems to follow from the work of Jersild, Burri and Hargreaves; though Jasper's and Notcutt's results are negative. Burri and Hargreaves make this factor distinct from the motor factor. There is no really unimpeachable evidence against this position, and other evidence, such as Jasper's and Notcutt's, is in agreement. Cushing has found a common factor in the play and other behavior of the pre-school child, while Bernstein has gotten a significant correlation between the ideomotor tests and the school behavior of children. Since Cushing's work did not, and could not, include the usual ideomotor tests, and Bernstein's results were contradicted by those of Sen Gupta, we must conclude that there is no real evidence for the extension of the 'p' factor to this sphere. Separate tests of perseveration in the emotional sphere have not been used, except by Cattell, who found his test part of the general unit; but his evidence was none too clear. Occasional emotional traits in some questionnaire or rating scale have been found related to the ideomotor tests of perseveration, as in the work of Reyburn and Taylor and that of Pinard. That the so-

called "volitional perseveration" of Downey does not constitute a unity has been shown by Dorcus; while Lankes, Pinard, Howard, Maginess, Cattell, and Clarke have shown that the type of perseveration manifested in the standard 'p' tests is either inversely or curvilinearly related to the characteristics that make for strength of character.*

The final answer to the question of the unity of perseveration may perhaps be given in the course of time by more general factorial studies with the methods of multiple-factor analysis. The results of those that have been made so far and that touch on perseveration seem to indicate that the answer will be that there are several factors entering into the various tests. Thus Line and Griffin have isolated a broad factor of "objectivity" which has a surprisingly high loading in the motor perseveration tests, while the second factor "fluency" or "mobility" has only a slight loading. Reyburn and Taylor have got a factor of perseveration which has almost the same loading in the 'p' tests as the "objectivity" of Line and Griffin. Rethlingshafer found that one of her factors, tenta-

* Cf. Spearman (79: II, 266-270). For the work of Maginess the present writer has no reference, nor is one given by Spearman.

Cattell's work (9) on relating perseveration to 'w' has not previously been mentioned in this review of the literature. His findings were similar to those of Pinard, which have been mentioned.

tentatively identified as perseveration, was present in some measures of ideational and motor perseveration but not in the classic paper-and-pencil tests. The latter were loaded appreciably with another factor, the habit of finishing what one starts. Perhaps this might have some relation to "objectivity." There was also a suggestion of other factors in the sensory and motor tests. Cattell, in his latest work, finds three factors: disposition-rigidity for the creative effort tests, mental perseveration for the intellectual tasks, and clinical perseveration for the recurrence effects, for tests more or less connected with natural tempo, and for some others.

We may say, in conclusion, that the vast amount of work on perseveration has been productive of very disappointing results in the end. Careful examination of the experimental procedure of many of the investigators leaves one under the impression that there was much to be desired in the way of exact control of conditions, exactitude of measurement, and rigor in applying statistical procedures. The paper-and-pencil tests have been among the most successful, or perhaps the least unsuccessful, but they are open to objection from several standpoints. The problem of scoring, which ought to have been settled before such a mass of work was done with the tests, is still such as to leave it doubtful whether the outcome of the tests is not merely an artifact of the particular scoring method chosen. All is not well in the house of perseveration.

CHAPTER IV

THE PSYCHOGALVANIC REFLEX (PGR)

Since the literature on the psychogalvanic reflex is vast and is only incidentally pertinent to this experiment, we shall content ourselves with mentioning only those phases of the general work on this phenomenon which have bearing on our problem or are helpful from the standpoint of method. There are just a few studies which concern the relation of perseveration to PGR.

The general literature has been very adequately covered up to 1932 by the reviews of Landis and DeWick (116) and Landis (113). A more recent summary may be found in Woodworth's manual (128). Farmer and Chambers (108), Cattell (98), and Darrow and Heath (106) also present a good portion of the literature, while Thouless (123) discusses the technical aspects of the work in some detail. The handiest summary of the data on the PGR in abnormal states is that of Landis (114). There is, however, no recent review of the literature -- at least to the present writer's knowledge. This is unfortunate, since considerable work has been done in recent years, especially from the standpoint of methods of measurement.

In observing changes in the electrical condition of the skin there are two basic phenomena to reckon with: the Feré

phenomenon and the Tarchanoff phenomenon. The former consists in a change of apparent resistance of the skin when an external voltage is applied to it. The latter is a change in electromotive force of internal origin, occurring under the same conditions under which the change in apparent resistance is found; it is measured by a circuit in which no external voltage is applied (116: 66; 128: 276 f.). Both phenomena are recorded by means of a galvanometer, are produced under similar stimulation, and are very similar in form. When an external voltage is applied to the surface of the skin, the Tarchanoff phenomenon is apparently not abolished, though its effect is obscured if the external potential is sufficiently high. Thouless (123) maintains that the required external potential at the electrodes affixed to the subject must be at least one volt in order to prevent the Tarchanoff phenomenon from distorting the Feré phenomenon.

Investigators have used a variety of circuits in measuring these phenomena. The most frequently employed has been some form of the Wheatstone bridge with a mirror galvanometer. In the standard Wheatstone arrangement, the subject is placed in the unknown arm and his apparent resistance can be measured by application of the ordinary formula for determining the unknown arm of the bridge. This circuit presents a difficulty in practical work, since the total resistance in the bridge will depend on the general level of the subject's

resistance. As a consequence, the reflex drop of the resistance will produce a greater swing of the galvanometer when the subject's resistance level before the drop is low than when his resistance level is high. This is inconvenient for purposes of measurement or even for a rough evaluation of the subject's reactivity.

This difficulty is obviated by the circuit described in Woodworth (128: 278) and by Darrow (101). In this arrangement a calibrated variable resistor is placed in series with the subject, resistor and subject thus constituting the fourth arm. The galvanometer is balanced by adjusting this variable resistor; a fall in the subject's resistance level may be compensated by an increase in the resistance in series with him, so that the total resistance in this arm (and hence the entire bridge) is constant. Naturally, however, the constancy must be destroyed while the reflex drop is occurring, but it is restored when the subject recovers. The current through the galvanometer is constant no matter what the subject's resistance level; each reflex decrease of resistance and increase of conductivity occurs as a certain percentage of the same level from drop to drop and from subject to subject.

There has been considerable debate about the nature of the physical change recorded by the galvanometer (116: 69-74). Though we are not concerned with this debate, there are several physical factors which must be mentioned because of

their bearing on method. It has been shown that the resistance level decreases as the subject remains in circuit. A wait of 10 to 15 minutes has been suggested as necessary before beginning work (116: 74-77). Our own preliminary procedure, as will be shown later, seems to indicate that this is not universally true. Muscular movement at the electrodes can undoubtedly distort the response. Gross movements of other parts of the body will in some cases cause a reflex. If the so-called dry electrodes are used, some means must be adopted of keeping the pressure on the electrodes constant, since a sudden increase of pressure will cause a swing of the galvanometer. There is some disagreement about the influence of temperature. Of interest to us, in view of our purpose of investigating the recovery time, is the contention of Gildemeister that local warming of the skin increases the duration of the reflex (116: 76).

Though the physiology of the response is by no means certain, it is beyond all reasonable doubt that the change in apparent skin resistance is due in some way to the activity of the autonomic nervous system. Innervation of the sweat glands is by way of the sympathetic division, though some parasympathetic connection is by no means excluded. If the innervation is exclusively sympathetic, then it is possible that these nerves in this case are functionally parasympathetic (103, 105). PGR does not correlate perfectly with other autonomic changes, such as variations in blood pressure, pulse rate, and vaso-

dilation or vasoconstriction (103, 105, 107). There is, however sufficient correlation to allow one to take this response as a rough index of general autonomic activity (112; 113: 732-34, 128: 283 f.).*

Many attempts have been made to relate PGR to character or personality traits. We shall mention only a few which will serve to illustrate the kind of results obtained in this field. It has frequently been assumed that the response indicates the presence of emotion and is its invariable concomitant; hence the response has been regarded by some as a possible index of emotionality. Wechsler (125) tried to determine whether emotionality is a general characteristic or specific to different situations. He came to the latter conclusion. Washburn and Pisek (124) have produced some inconclusive evidence that cheerful and emotional subjects respond more intensely than depressed or nonemotional subjects. Porter and Copeland (120) interpreted their finding that girls are more reactive than boys from 15 years to and including college age to mean that the former are more emotional. Landis (113: 729), however, pointed out that the greater reaction may be merely

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Landis seems to be overstating the case when he says that Darrow has shown that PGR and blood pressure vary independently (113: 734). That worker and his associate Solomon have shown rather that the two indices do not exhibit the same degree of change; that when one reaction is pronounced the other may be mild (100, 107).

a function of the greater metabolic rate of women, since PGR is positively related to this factor. Syz (121) maintains that the frequency of the psychogalvanic response is significantly related to differences in emotionality among groups, but is too variable an index for the study of individual differences in emotionality.

Against this evidence may be balanced the results of other investigators. W. S. Brown (96) found no correlation of PGR with teachers' estimates of the emotionality of their students. Neither emotional stability nor the reported intensity of emotion is related to the intensity of PGR according to the results of Talenti's study (122). Landis (115) in a study of 100 delinquent boys, found no significant relationship of frequency or latency of response with any other measure of emotionality.

Correlations of .44 with ratings for magnetic personality, and of .40 for nervous temperament ratings, were reported by Fleming (109); but there were only 18 subjects. Linde, according to Landis (113: 728), noted that introverts tend to give a galvanometer record characterized by a smooth, swinging curve of response, while extroverts tend to give sharper, peaked curves.* In the careful research of Darrow

* The present writer was unable to find this statement in the article referred to by Landis (119).

and Heath only one of several measures of galvanometric response showed anything approaching a significant correlation with introversion-extraversion score. This was the degree of spontaneous reactivity during two minutes of anticipation; the correlation was .261, which was not significant (106: 117 f.). Two measures, total resistance drop during two minutes of anticipation and the recovery within two seconds after a reflex drop, correlated negatively and significantly with unfavorable health characteristics (106: 113, 168). Two measures, rise in resistance during two minutes of rest and the degree to which the reaction was subject to conditioning, correlated significantly and negatively with neurasthenic tendencies (106: 188 f., 205 f.). None of the correlations exceeded .310; all others -- for quite a variety of PGR and personality measures -- were lower and non-significant.

There has, to the present writer's knowledge, been only one investigation in which the psychogalvanic reflex has been used in connection with any of the usual tests of perseveration. Cattell, in his early study of temperament tests and temperament factors (9) which we have mentioned in the discussion of perseveration as a unit factor, included a measure derived from the galvanic skin response. Though he does not say so, it appears from a reference in his account that he measured the extent of the drop in terms of the per cent of the

level of resistance just before the drop.* Since he fails to report a correlation between this measure and perseveration, although he is at great pains to mention a series of other correlations which are low and not significant, we may take it as probable that he found none to speak of.

There have been two studies in which a characteristic of the psychogalvanic response has been taken directly as a measure of perseveration, without reference to the usual tests of that function. Mays (56) wished to find out whether there is, corresponding to the tendency to pathological perseveration found among catatonics, a similar tendency of the autonomic nervous system toward repetitive activity. He assumed that, if there is such a tendency, it should be manifested in a tendency to maintain the same magnitude of psychogalvanic response with repetition of the same stimulus; whereas in normal subjects, as is well known, adaptation quickly sets in and the response diminished. The index of perseveration of psychogalvanic response, therefore, was the ratio of later to earlier responses in the same session; and also the ratio of responses in a later session to those in the first session. He found catatonics more perseverative than normals according to both these measures.

* His reference is apparently faulty. He probably meant to refer to another article of his (98).

Shipley (76) conceived of perseveration as a continued set to act in a certain way, and conjectured that it ought to result in a lowering of the threshold to stimuli in general. To the measure of resistance to adaptation he added three others: 1) susceptibility to conditioning of the skin reflex, 2) susceptibility to experimental extinction, and 3) the degree of irradiation. Comparing the responses of schizophrenics (non-catatonic), manic-depressives, psychoneurotics, and normals, he found the schizophrenics highest, i.e., most perseverative, in three of the measures, lowest in the fourth. Resistance to extinction was the exception. When the four scores were suitably weighted and combined, the order of mean perseveration score was (highest to lowest): schizophrenics, psychoneurotics, manic-depressives, and normals. Since the samples were all small, the greatest number of cases being 17 for the schizophrenic group, these differences were not significant, except for that between the two extremes: schizophrenics and normals.

Travis and Knott (85, 86) have carried the investigation of perseveration into the field of brain potential measurements. They are mentioned in this place because of the similarity between their approach and that which we have attempted in the use of the recovery time with PGR. They have published two studies in which they investigated the perseveration time to light and to verbal stimuli visually presented. Upon pre-

sensation of a stimulus, the slower Berger waves are replaced by the faster Alpha waves; after the stimulus is removed, the slower waves gradually make their appearance again. There were fairly wide individual differences in the average time required before the reappearance of the slower waves, though overlap was considerable. With light as the stimulus, the range of means was .70" to 1.43"; the lowest S.D. was .23", while the highest was .53".

Before leaving the subject of PGR, we must consider evidence relative to the important questions of the possibility of finding individual differences in this matter, and of the reliability of the measures. Cattell (98) found great individual differences in the extent of the deflection; the curve shape, however, while constant for a given subject at a single sitting, varied considerably from day to day. Farmer and Chambers (104) maintain that PGR is not a reliable measure for individual differences, but only for group tendencies. They do not, however, present substantiating data.

In regard to reliability, Lauer (118) obtained raw test-retest correlations of .619 for a measure of the extent of deflection; .522 for the change in ohms; .552 for the per cent change. (What the difference is between the first two is not clear from his account.) These coefficients were stepped up to .78, .72, and .74, but the statistical procedure is not clearly indicated. Wechsler, Crabbs, and Freeman (126) retested 19 children after a lapse of four to eight weeks. The test-retest

coefficient (ρ) for the median amplitude of response was .727 (P.E.: .076); for the ratio of responses over total time, .194 (P.E.: .160). We shall evaluate these findings later in reference to our own work, but we may note here that they are based on only 22 and 19 subjects respectively, so that at best they would hardly serve as a guarantee of reliability of the measure in general. Welch and Kubis (127), using a measure of ease of conditioning of the response, obtained the high value of .88 for the correlation between the first tests and a repetition one month later. This was with 36 normal subjects.

CHAPTER V

THE PROBLEM

One reason for confining this investigation to a limited area has been indicated briefly in Chapter I. But we must explain the purpose of this study more fully. In the theories of Mueller and Pilzecker, the Dutch School, and the Spearman School, perseveration has been regarded as due to or identical with a fundamental characteristic of the nervous system. To test this hypothesis directly, both for the sensory and the autonomic nervous system, was our first intention. It appeared to the present writer that the so-called direct tests of sensory perseveration were either not direct enough or could be improved on, and he hoped at first to be able to devise better tests. However, the difficulties encountered with the psychogalvanometer led him to concentrate on this measure of autonomic activity and leave the other work for future research if the results of the present study turned out positive.

Another reason for contemplating in the first place an investigation of persistent or continued functioning of the autonomic nervous system lay in the fact that many of the various manifestations of introspected perseveration involve emotions, moods, or experiences that are emotionally toned in some

way. Hence the idea suggested itself that there might be some connection between this fact and the activity of the autonomies which are involved in the total emotional experience. The reason for planning a measure of only the degree to which the autonomies continue to function after being once aroused was determined largely by the emphasis placed on continuance of function in Spearman's theory, with which the present writer was principally concerned. Though, as will be seen, repetitive or recurrence phenomena are included in the perseveration questionnaire, these are related to the continuance phenomena, so that a separate measure is not necessary. The interference phenomena, such as the difficulty in rapidly changing a train of thought, seem theoretically to be corollaries of the continuance phenomena and are also shown by the results of the analysis of the questionnaire items to be connected with them.

The psychogalvanic reflex was chosen as the measure of autonomic activity because it is the most sensitive and the most susceptible to measurement. The degree of reactivity, in terms of average reflex drop in resistance, did not appear to be the appropriate measure, since the question was not one of intensity of reaction, but rather duration. Hence some measure of duration was sought. As we shall see later, getting such a measure was not as easy as it seemed at first sight.

It may be objected that the PGR does not correlate perfectly with other measures of autonomic activity. True enough, but it is as good a measure as we have at present. It

may also be objected that it does not reflect the activity of the entire autonomic system, but only that of the sympathetic division. This is by no means certain (105), but must be considered as a possibility. There is, however, good evidence that the reflex is functionally associated with the parasympathetics, though the sweat glands, its principal organic mechanism, are anatomically innervated by sympathetic fibers. Hence informed opinion inclines to the view that both systems are operative in eliciting PGR (112).

The term perseveration may be used to designate either a tendency to persistent or recurrent activity or the actual manifestation of that tendency. For the purposes of this study, we shall define it in the former sense as the tendency of certain psychophysical or purely mental acts, such as sensations, words, phrases, tunes, motor activities, ideas, and emotions, both to persist after the cessation of their exciting stimulus and, after once ceasing, to recur spontaneously. We have not mentioned the interference or difficulty felt in changing from one activity to another, since this appears to be but the natural consequence of persistence and recurrence.

The psychogalvanic response (PGR) may be defined as the sharp drop in apparent skin resistance upon the presentation of a sensory stimulus, the occurrence of an idea, the arousal of an emotion, or some other mental or physiological change. The sharpness of drop is relative to the preceding

state of the skin resistance. If this has been constant, the response will consist in an easily distinguishable and comparatively quick change. If the resistance has been falling, even though the rate of change has been relatively rapid, the response may be identified as a sudden increase in the rate of drop. A gradual, steadily progressing lowering of the resistance, whatever its cause, is not included in the term psychogalvanic response. Various apparent causes of the response have been mentioned in the definition, since the reaction, whatever its proper cause, is in actual fact associated with a wide variety of activities (128: 294 ff.). Though many of these stimuli or changes will not invariably produce a drop in skin resistance, they are capable of doing so. The terms galvanic skin response (GSR) and skin reflex we consider synonymous with PGR.

We may delimit the problem investigated in this study by the following series of steps: 1) The theory has been proposed that perseveration is due to a general tendency of one's nervous system to persist in activity, continuously or intermittently, when it has once been aroused to a certain type of activity. 2) If this is true of the nervous system universally, then it is true also of the autonomic system. 3) Consequently there should be a correspondence between the observable manifestations of perseveration and the duration of activity of this segment of the nervous system. If, in a certain individual, perseveration is marked, so that sensations, ideas, emotions, etc., tend to last long after their apparent cause has ceased,

then the responses of the autonomies should also tend to last long. Measures of the two should show a significant correlation.

We attempt, therefore, to measure perseveration as introspectively manifested and the duration of autonomic response in a number of individuals to see if such a correlation is to be found. A questionnaire is used as the measure of perseveration; the duration of the PGR, as the measure of the duration of the autonomic activity. In both cases necessity has forced us to make the measure somewhat more indirect than we should like.

In conclusion, the basic hypothesis which we wish to test may be formulated as follows: Perseveration is dependent on a general tendency of the entire nervous system to persist in activity once it has been aroused. Negative results, i.e., to the effect that this alleged dependence is not true, will be unfavorable to the general hypothesis; positive results, i.e., to the effect that the dependence is a fact, will only partially confirm the general hypothesis. The experiment is crucial only in this sense.

If one wishes to insist that PGR is indicative only of sympathetic activity, the bearing of the experiment on the main hypothesis is not materially changed, since the universality of this supposed tendency is the thing in question. The specific hypothesis may then be modified by substituting sympathetic for autonomic in the above statement.

CHAPTER VI

PRELIMINARY EXPERIMENTATION

The preliminary work for this investigation concerned, first, the construction and standardization of a perseveration questionnaire, and secondly, experimentation with galvanometric techniques. The latter work may be divided into two parts: 1) a preliminary survey to determine the feasibility of our approach, and 2) a test of the effectiveness of certain stimuli and of the possibility of obtaining a reliable measure of the duration of the psychogalvanic response.

A. CONSTRUCTION AND STANDARDIZATION OF THE QUESTIONNAIRE

Construction

In constructing the questionnaire, items were sought which represented phenomena rather commonly accepted as perseverative. For this reason, the majority of the questions included were derived from questionnaires previously published by Lankes, Jasper, and R. B. Cattell.

Lankes' questionnaire (53) was the first in order of appearance and served more or less as the basis for the other two. While it would be of little interest to reproduce his items (there were 17), mention may be made of some typical questions and the method of answering and scoring them. The

first item reads:

1. Do you often notice a tune, line of poetry, phrase, problem, etc. coming back to your mind again and again without your intending it? How often (about) a week? At what time of day more frequently?

The subject was instructed to answer: "yes, no, very much, never", or to make some other short but clear response as the question demanded. The score was simply the sum of the answers that indicated perseveration. Standardization was attempted on the basis of agreement with perseveration score on other tests, with the result that several items were eliminated and weights determined for those retained.

Jasper's questionnaire (38) was simpler in form.

There were 21 items which called for a yes or no answer and were generally briefer and clearer. For example:

1. If you have been disappointed, do you get over it easily?
2. Are you inclined to worry about things?

Again, the score was merely the sum of the perseverative answers. No attempt was made at standardization, except to calculate the odd-even correlation, which was .59, (P. E.: .06) when corrected by the Spearman-Brown formula.

R. B. Cattell's test (9) consisted of 17 questions to which no set answers were provided. The spontaneous answers were scored on a scale of 1 to 3 in as far as they indicated no, a moderate degree, or decided perseveration. The first two items are representative of the author's wording:

1. When you have made up your mind to do a certain piece of work, do you sometimes find yourself persisting with it even after the causes for doing so have disappeared?
2. In conversation, can you pass quickly from one subject to another, or do you find it more natural to exhaust one subject at a time?

The questions were evaluated in terms of distinguishing those who were classed as high, moderate, or low perseverators, according to tests of motor perseveration.

A number of items suggestive of perseveration were discovered in the Minnesota Multiphasic Personality Inventory (130). Many of these were already represented equivalently in the above questionnaires, and others were of doubtful application. Hence only the following two items were deemed usable:

I cannot keep my mind on one thing.
I find it hard to keep my mind on a task or job.

They were combined into one and modified to conform to the general pattern of the other items.

The actual construction of the questionnaire involved several stages. A sort of pool was established in which questions were formulated as briefly and clearly as possible, and classified under four heads in as far as they appeared to the experimenter to involve experiences primarily cognitive, or to include an element of interest besides mere cognition, or to pertain to the interference phenomenon found in change of habit and switching from one activity to another, or, lastly, to involve emotions or emotion-toned acts. For example, the question, "Do you dream about things that have recently happened?"

was considered primarily cognitive; the question, "If you have some worry, does it keep coming back to mind when you don't want to think of it?" was classed as primarily emotional. Since it was not possible to amass or use an equal number of items of each type, this classification was given no attention after the formulation of the first preliminary questionnaire, though it is hoped, at some future date, to make use of this material.

The first form referred to consisted of 24 items, all of which were based on the sources listed though often considerably modified. After each question there were four possible answers, among which the subject was to choose the one that fitted him most nearly. The answers were then scored from 0 to 3 according to the degree of the particular phenomenon in question. Frequency of occurrence formed the criterion for the most part, since most of the answers read: "never, occasionally, fairly often, very often."

After this form had been administered to a number of graduate students and discussed with them, another was constructed on the basis of their criticisms and suggestions. In this form there were 38 items, many of them suggested by the critics. In order to allow greater discrimination and approximate more to equality of scale intervals, the number of possible answers was increased to six, so that the item scores ranged from 0 to 5. Because of difficulties encountered in filling out the questionnaire and in improving the wording of

both questions and answers, it was decided to adhere to frequency of occurrence as the basis of scaling for the most part. As a consequence all but five of the item-answers were worded: "Never, seldom, occasionally, often, very often," or in some similar fashion. In some cases, greater precision was attempted. This second questionnaire was given to the same group and again thoroughly criticized. Greater discrimination was obtained, but further difficulties were encountered. Aside from ambiguities, the principal trouble lay in the attempts to make the answers too precise. For example, the following was one of the troublemakers:

20. If something goes wrong early in the day, does it put you in a bad mood?
☐ Hardly at all. ☐ For a few minutes. ☐ For about a half-hour. ☐ For about an hour. ☐ For several hours. ☐ About the whole day.

It was feared also that arranging the answers in order from least to most perseverative or vice versa would invite routine checking of the middle position; hence, in the final form the answers were placed in random order, with care to keep the first and last positions about equally divided between the various scale values.

The final version of the questionnaire took the form reproduced in Appendix I. The scale values have been inserted for the sake of clarifying the scoring method. The total score was simply the sum of the values earned on the 40 items. It will be noted that some of the questions require answers which

are different from the "never-very often" pattern or which have to be scaled in the opposite direction; i.e., "never" indicates high perseveration instead of low; and "very often", low perseveration instead of high. While largely determined by the nature of the particular phenomenon sampled by the question, this was welcomed as a means of counteracting the tendency to hurry down the list and check the same answer for each item.

Standardization

The Likert technique (132) of internal consistency was used as a preliminary method of standardization. This method is the same as that used by the Thurstones in preparing their Thurstone Personality Schedule (140); hence we have good precedent for applying it to a trait test. According to Likert's description of the method as applied to attitude tests, one first prepares a set of items relative to a certain attitude, and provides several responses to each item which indicate various degrees of agreement or disagreement and can be scaled from 1 to 3, 1 to 5, and so on. After the test has been administered to a large group, the item scores are summed in the usual manner and the test protocols arranged in the order of ascending total score. The upper and lower ten percent according to total score are then selected. An item analysis is then conducted by computing the average score earned by each of these two groups on each item, and by finding the difference between these two averages on each item. If an item shows very little difference, it is

not discriminative and should be eliminated as contributing little to the scale. If the difference is negative: i.e., if the upper ten per cent by total score have an average on a particular item lower than that of the lower ten per cent by total score, the item has been scored in the wrong direction; if the difference, however, is great enough to warrant retaining the item, its wording or its scaling should be reversed. The criterion for determining whether or not the difference is sufficiently great for discriminative purposes is not explained by Likert to the present author's satisfaction. It appears that the choice of a criterion is arbitrary.* That the usual procedure of determining a statistically significant difference does not serve one here, will be shown later.

It can be seen that this method is primarily designed to make the scale more discriminative and to avoid errors due to items that measure in the wrong direction. However, it does perform a function analogous to that of the correlation method in that one cannot have a high linear correlation between items that show only a small difference between the two extreme groups, unless the scale contains very minute steps and is highly accurate. This cannot be achieved in tests of

* Nor is this matter cleared up in a later article written in conjunction with Roslow and G. Murphy (133). In the work there described, the upper and lower extremes constituted 25 per cent.

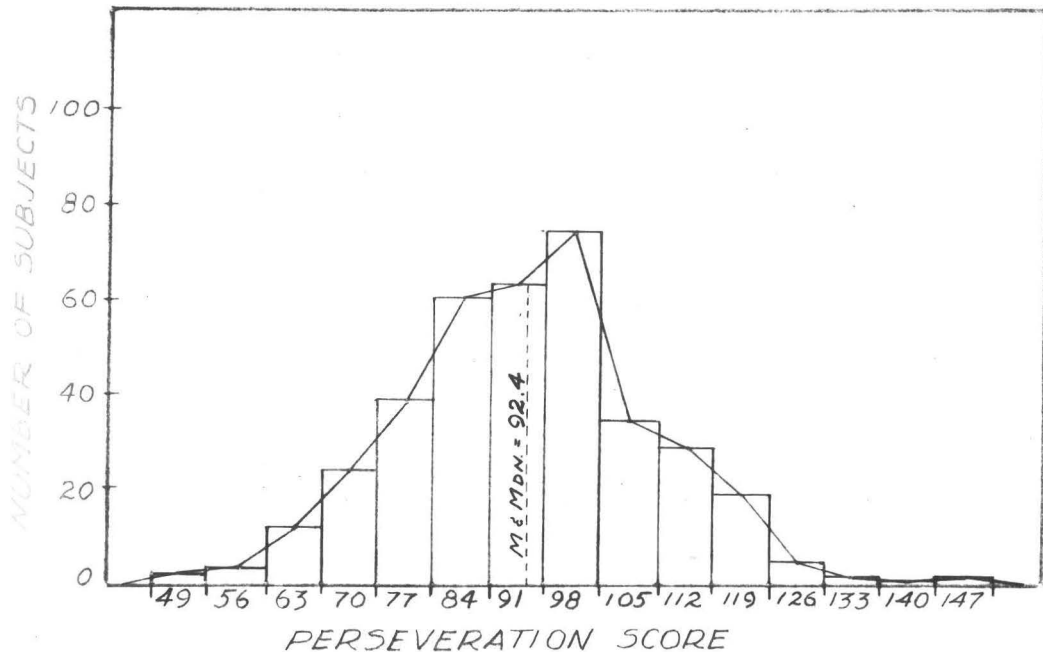
this kind. Hence one can make a relatively quick survey of the items and eliminate many of those that will yield nonsignificant correlations. Again, those items which discriminate in the wrong direction would almost certainly show a negative correlation, and would have to be discarded or reversed anyway.

The method will become clearer from the following report of the results of its application. The questionnaire was administered by the experimenter to some 500 college students at Loyola University in the fall and winter of 1946-47. Academically they ranged from freshmen to graduate and special students, the latter being comparatively few. A group of senior student nurses was also included. The age range, 17-0 to 56-10, was wider than that usually encountered in college groups. The men numbered 359; the women, 103; three subjects did not indicate their sex. Despite the provision of space for the name at the top of the questionnaire, the subjects were instructed not to put their names on the paper, but rather to write down their sex. For the group subsequently drawn on for the first preliminary work with the galvanometer, code numbers were assigned.

The mean score of the normative group was 92.42; the median, 92.44; the range, 46-148; the standard deviation, 15.61; the standard error of the mean, .73. The distribution, as can be seen from Fig. 1, approaches the normal.

FIGURE 1.

DISTRIBUTION OF PERSEVERATION SCORES
ON QUESTIONNAIRE (N=463)



Since some of the test records had to be rejected because of failure to answer all the questions or the marking of two answers to the same question, the number available for analysis was 463. From this test pool, the experimenter selected the 43 protocols which showed the lowest total score, and the 44 which showed the highest total score. Actually, these constituted 9.3 and 9.5 per cent of the entire distribution. This was necessary in order to avoid drawing the lines of demarcation at points where ties occurred, while yet

keeping as close to the ten per cent ideal as possible. For convenience, these groups will be referred to as the highest and lowest ten per cent, despite the discrepancy.

Upon selection of the highest and lowest ten per cent, the score made by each of these individuals on each item was tabulated. Then the average score on each item was computed for the 43 in the low group and the 44 in the high group. The difference between these averages was obtained by subtracting that of the low group from that of the high group. All differences but two proved positive; that is, the high group scored higher than the low group on all items but two. The results of this procedure are presented in Table I.

It is clear at once that some of the items discriminate poorly between the high and the low groups. However, it is not easy to decide precisely where to draw the line. Evaluating the differences statistically would be of little help. An item may prove to differentiate significantly, yet yield such a small difference that it does not contribute much to the discriminatory value of the scale. It appears necessary, therefore, to set an arbitrary criterion and check for statistical significance those items that lie at or just above this criterion.

TABLE I

APPLICATION OF LIKERT TECHNIQUE TO THE QUESTIONNAIRE:
MEAN ITEM SCORES AND DIFFERENCES

Item	Highest 10 Per Cent	Lowest 10 Per Cent	Difference
1	3.21	1.77	1.44
2	4.02	2.56	1.46
3	2.25	.33	1.92
4	1.70	.14	1.56
5	3.36	1.19	2.17
6	3.77	2.30	1.47
7	2.77	1.28	1.49
8	3.14	1.67	1.47
9	2.66	1.00	1.66
10	2.34	1.86	.48
11	1.25	.35	.90
12	3.91	1.60	2.31
13	3.73	1.51	2.22
14	2.52	1.56	.96
15	2.45	.53	1.92
16	2.70	.65	2.05
17	2.70	.79	1.91
18	2.73	1.16	1.57
19	2.20	.65	1.55
20	2.68	.95	1.73
21	4.23	1.91	2.32
22	2.52	.88	1.64
23	2.77	.98	1.79
24	2.75	2.70	.05
25	1.48	.44	1.04
26	4.23	2.35	1.88
27	1.68	.53	1.15
28	4.23	3.07	1.16
29	2.91	1.81	1.10
30	3.32	1.79	1.53

TABLE (CONT.)

Item	Highest 10 Per Cent	Lowest 10 Per Cent	Difference
31	3.57	2.65	.92
32	4.05	3.37	.68
33	4.39	3.19	1.20
34	4.57	1.58	2.99
35	2.05	2.67	-.62
36	4.00	2.44	1.56
37	2.86	3.40	-.54
38	3.32	1.88	1.44
39	3.36	1.88	1.48
40	<u>2.52</u>	<u>1.91</u>	<u>.61</u>
Total	120.90	65.33	56.68
Mean	3.02	1.38	1.42*

* This mean difference is calculated from the arithmetical sum immediately above it. Slightly different means are obtained (1.35-1.64) with other ways of calculating. The discrepancy is due to rounding.

Following this course, we decided on 1.44 as the minimum difference acceptable because it is close to the figure actually used successfully by Likert, seems safe statistically, and allows us to retain 26 items. When it was computed for the items showing a difference of 1.44 and 1.46, the significance levels were better than 0.1 per cent. This is quite adequate. The following items were, therefore, retained. Nos. 1, 2, 3, 4, 5, 6, 7, 8, 9, 12, 13, 15, 16, 17, 18, 19, 20, 21, 22, 23, 26, 30, 34, 36, 38, and 39.

The obtained differences might reasonably have served as a basis of weighting the various item scores. This, however, was not done for two reasons. In the first place, it would make the scoring very cumbersome and laborious. Secondly, it would entail the risk of exaggerating the inequality of the scale units within the range of answers to each item. It is not certain, for example, that the difference between the responses, "never" and "seldom", is the same as the difference between "seldom" and "occasionally." There is here a source of inaccuracy which we should prefer to leave as it stands, rather than take the chance of increasing it by weighting items, the scale intervals of which we do not know. Putting the responses to each item on a normal scale would be an answer to this latter difficulty, but it would also make the scoring very cumbersome. At any rate, Likert's experience in comparing his technique of internal consistency with Thurstone's method of scaling seems

to be adequate evidence that not much is gained by using the normal scale.

As a further check on internal consistency, a sample of 200 test protocols was selected from the 463. Intact groups were used: the final experimental group and two others which were chosen because of their similarity to the experimental group and also because the experimenter had greater assurance than in the other instances that these tests had been filled out carefully. In one of the groups a number of records were eliminated at random so as to keep the total exactly at 200 for ease of computation. One record was excluded from the experimental group because there was some doubt as to the subject's care and reliability in responding to the questions. Correlations were then run between each of the 26 surviving items with the total score after the particular item score had been subtracted. This last procedure was adopted so as to prevent spurious correlations. The results are presented in Table II. Since there were only six intervals along the one axis of the correlation chart, the correction for broad categories was applied; these are designated as c_r in the table. The statistics t and P are given only for the ten lowest correlations. P is taken for only one tail of the distribution. The standard error for all the

TABLE II
CORRELATIONS OF EACH QUESTIONNAIRE ITEM
WITH TOTAL SCORE MINUS THE ITEM*

Item	<u>r</u>	<u>cr</u>	<u>t</u>	P
1	.099	.108	1.52	.129
2	.194	.211	2.97	.003
3	.351	.382		
4	.476	.518		
5	.434	.472		
6	.188	.204	2.87	.003
7	.181	.197	2.77	.006
8	.311	.338		
9	.127	.138	1.94	.052
12	.408	.444		
13	.488	.537		
15	.294	.320		
16	.499	.543		
17	.448	.488		
18	.242	.263	3.70	.0002
19	.260	.283		
20	.412	.448		
21	.429	.467		
22	.441	.480		
23	.413	.449		
26	.076	.083	1.17	.242
30	.143	.156	2.20	.028
34	.435	.469		
36	.130	.141	1.99	.047
38	.255	.277	3.90	.0001
39	.256	.278		

* cr is the correlation corrected for broad categories. The standard error of all the correlations is .071; t and P are given for only the ten lowest corrected correlations.

correlations is .071.*

The item correlations are low. However, all but five of the corrected coefficients are significant at the 1 per cent level or better. Of these five, three reach (or practically reach) the five per cent level, while two are clearly not significant. Low correlations were to be expected, since the range within each item is extremely limited and since each item is equivalently a very short subtest.

Another possible interpretation of our low correlations may be that we are dealing with a relatively variable tendency which finds different modes of manifestation in different individuals. Assuming that the total score is a rough indication of the strength of this tendency, it is not impossible that quantitatively similar tendencies in two individuals may exhibit distinct qualitative characteristics. That is, concretely, one person may experience perseverative tendency in the constant urge to hum a tune; another, in the urge to repeat phrases which have caught his fancy. This explanation cannot, of course, be substantiated by appeal to our data; but it remains a possibility.

* In computing the standard error for correlations we shall use the formula given by Peters and Van Voorhies (137: 153) which is appropriate for testing the hypothesis that the true correlation is zero. Since the result thus derived will depend only on the number of cases, any differences in the standard errors given in any of our tables will indicate that the number of cases has been changed.

On the basis of these correlations, it would appear that Items 1 and 26 should be eliminated. This was not done, however, because of the following situation. When sample split-half correlations were run with the 52 records from the experimental group, the test showed a higher reliability with these items included than without them. With these items, the raw coefficient was .689, which became .816 when corrected by the Spearman-Brown formula (137: 194). Without these items, the raw coefficient was .531; the corrected, .694.* Both these items had passed muster according to the Likert technique; Item 26 had shown one of the larger differences (1.88), while Item 1 had shown a difference (1.44) which was still significant at the 0.1 per cent level. It appears, therefore, that we have two reasons in favor of retaining these items as against one for rejecting them. They have, therefore, been retained.

Although we are primarily interested in the reliability of the test as actually found with the group used in the final experiment, the coefficient obtained from a larger sample is also of interest. With the 200 records used for the item correlations, the raw odd-even correlation was .669, which rose to .802 upon correction by the Spearman-Brown formula. The predicted correlation with a test of infinite length was .898.

* The predicted coefficients of correlation with a test of infinite length were .901 and .829 respectively (137: 195).

What the reason is for the lower figure with the larger group, we cannot say. At any rate, taking the corrected correlations (.816 and .802) as probably best indicating the true reliability of the test, we may say that the questionnaire is fairly reliable. We should, however, like to see it somewhat higher.

The question of validity remains to be discussed. We have at present no way of demonstrating the validity of this questionnaire. We cannot use judges' estimates as a criterion, since these would be, if anything, more uncertain than the subjects' self-ratings. We cannot use the so-called objective tests of perseveration, since these are by no means beyond criticism. The only method that, to the present writer, seems applicable is the method of a criterion group. This would mean trying the test out on psychotics who showed the psychiatric symptom of perseveration. Whether this could be done successfully is doubtful. The degree of accurate introspection demanded of the testee in this case seems to forbid the use of psychotics. Our justification for calling this a questionnaire of perseveration lies therefore in no external norm, but solely in the evidence of internal consistency. A number of phenomena have been sampled, of which the majority have been agreed upon as belonging to the same class. The two methods of item analysis show that the retained items tend to go together and hence measure roughly the same thing. We do not pretend that we have more than a rough measure, though we think it is fairly good as such measures go.

Our final conclusion, therefore, from our work in standardizing this questionnaire is that it is a rather discriminative measure allowing for wide individual differences; that it is fairly reliable at least in regard to the group for which it was designed, i. e. for the subjects of the final experiment with PGR; that it is probably a valid test of what it purports to measure, though it is impossible to get a numerical estimate of its validity.

B. PRELIMINARY WORK WITH THE PSYCHO GALVANOMETER

The investigation of the relation between perseveration as measured by the questionnaire and the duration of the psychogalvanic response necessitated extensive preliminary work. This we shall describe in its main outlines, omitting many of the details involved in achieving a satisfactory procedure and the proper mechanical and electrical devices. The aim of this work was at first merely to get some idea of the type of results to be expected, so as to judge whether the experiment was likely to yield meaningful results. As it turned out, further effort had to be directed to perfecting the psychogalvanometric technique so as to obtain some assurance that we would be able to measure the duration of response in some reliable way.

1. The First Stage: Survey

The perseveration questionnaire was administered to the 92 members of the experimenter's class in general psychology. These subjects were also part of the normative group. In

academic standing they ranged from freshmen to juniors. Their ages ranged from 17-7 to 30-1, with the mean at 21-9 and the standard deviation at 2-7. The questionnaires were scored as previously described; all 40 items were used, since the work of standardization had not yet been completed. Nine subjects (i.e. approximately ten per cent) were chosen from the higher extreme of the distribution and nine from the lower. They were then requested to go through the galvanometric experiment.

Stimulus Material

The means chosen to elicit a response was word association. A list of twenty words was prepared, the ten most emotionally-toned and the ten least emotionally-toned in Whately Smith's list (128: 289). The words were arranged in this order: "swim, pencil, pond, flower, give; kiss, love, marry, divorce, S's first name; glass, white, hunger, bury, carrot; woman, wound, dance, afraid, proud." The first and third groups of five had given the least deflections in Smith's work, the second and fourth groups of five had given the greatest deflections. After a few trials, this list was shortened by the elimination of the first ten words for two reasons. The length of the session was so great, about two hours, that results toward the end were very unsatisfactory; and secondly, the pile-up of the connected words, "kiss, love, marry, divorce," elicited responses which, though innocent enough, apparently caused the subjects to fear that their instructor might regard them as girl-crazy.

That this was not an empty surmise on the part of the experimenter was indicated by a remark of one of the subjects. This worry seemed to delay recovery from the response and to cause sudden drops in the midst of the recovery curve.*

Apparatus

The apparatus consisted of a Wheatstone bridge, a mirror galvanometer, a photographic recorder, and an exposure box. The standard Leeds and Northrop Potentiometer #257795 and their mirror galvanometer #200318 were used, the latter provided with shunts of 25, 50, and 100 ohms. A lamp was mounted in the galvanometer box to one side so as to provide a record of the time at which the stimulus was presented and also the response time. In place of a special lamp to mark seconds on the record, the galvanometer lamp was interrupted once per second. Instead, therefore, of the usual solid-line record of resistance change, there was a dotted line. The recording apparatus consisted of a drum, mounted in a light-tight box and driven by a telechron-clock motor, which drew standard, low-speed six centimeter electrocardiograph paper past a narrow, horizontal slot at a speed of 20 seconds to the inch. The box was so mounted that the narrow slot was only one-quarter inch from the opening in the galvanometer box, from which the ground-glass scale had been removed. The exposure box was similar to a drop tachisto-

* Hereafter we shall call these "secondary drops," since they occur after the main drop but before complete recovery. Roughly they occur within 20 seconds after the main drop.

scope, except that its face was at a 45° angle and the window remained open until the experimenter closed it manually, immediately after the response had been given. This box was wired in series with a power source, a telegraph key, and the special lamp in the galvanometer box. Before exposing a word, the experimenter held down the key, so that, as soon as the falling window closed a set of contacts in the box, the lamp was activated. As soon as the response was given, the experimenter released the telegraph key, breaking the circuit. The onset of the stimulus was thus indicated by the leading edge of a grey band at one side of the record; the giving of the response, by the trailing edge of the grey band. The word list was thus presented visually, each word being clearly printed in letters one-half inch high on a card five by three and one-half inches.

The subject electrodes consisted of two cups filled to a convenient level with a one-tenth normal solution of sodium chloride. No attempt was made to keep constant from person to person the amount of skin in contact with the electrolyte. This would involve a complicated procedure which would allow for the varying thickness of different persons' fingers, and would be required, as far as the experimenter can see, only if one wanted to compare resistance levels very accurately. Three volts were delivered to the bridge circuit from two flashlight batteries. The voltage at the galvanometer was not constant, nor was the amperage, since the total resistance in the bridge varied with

the subject's resistance.

Procedure

The subject was seated before the exposure box at a distance convenient for reading. A screen just above the box prevented his seeing the apparatus or the movements of the experimenter. The digit and middle fingers of his left hand were immersed in the cups. The battery switch was closed immediately so as to put the subject in circuit without delay. The bridge was balanced so that the initial resistance could be found, and the following instructions were given:

Assume as relaxed a position as possible. Place your left forefinger and middle finger in the cups, allowing the bases of the fingers to rest lightly but securely upon the near edges of the cups.

Do not remove either finger from the cup until the experimenter tells you at the end of the experiment. This is important to protect the instrument.

A list of words will be presented to you one at a time. You are to respond with the first word that comes to your mind. Do not make an effort to seek any particular kind of word. Regardless of what it is, do not reject the first word that comes to mind.

Before each word you will be given a ready signal. Fixate your gaze on the center of the exposure-box window. After a pause, a word will be exposed. From the moment of the ready signal until I say, "At ease," try to avoid any motion of hand or body, and also to avoid any laugh, sigh, cough, sneeze, or a breath deeper than usual. If anything of this sort occurs, report it to me after the signal, "At ease." Do not talk during the experimental period itself.

After the entire experiment is over, you will have a chance to explain your various feelings and word associations if you wish. Please indicate whether or not your health seems to be as good as usual, whether or not you are fatigued.

An adaptation period of about 15 minutes intervened. After the first half of the word-list had been discarded, some of these

words were used for practice, though no record was made of the associations or reactions. The adaptation period was terminated when the experimenter judged that the subject had reached a stable level of resistance or that it was unlikely he would reach such a level for an indefinite period. The subject was then given an opportunity to shift his position, and the instructions were repeated synoptically. Rest periods were introduced during the experimental series, according to the length of the session and the apparent need of the subject. As a rule there were two rest periods: after the third or fourth word, and after the seventh or eighth. It was usually necessary to wait several minutes after a rest period for a subject to settle down. The word associations given orally by the subject were noted by the experimenter, together with the resistance level as the bridge was balanced and re-balanced. At the end of the experiment, introspections were taken, and information obtained on the following points: general health, fatigue condition, emotional strain, tension because of the experiment, explanations of unusual associations, tension especially at the ready signal, consciousness of an emotion on the several words.

Results

Before any attempt was made to analyze the records quantitatively, it was apparent that there was a fairly consistent pattern of response for each subject and that many of them differed widely in this regard. This fact was the principal consideration in our determination to go ahead with the experi-

ment in the face of the doubtful quantitative results, which will be discussed in a moment. The introspections were not analyzed because they were too vague and unreliable. Analysis of the associations was not contemplated in the design of the experiment, nor did it seem useful with only ten responses.

The quantitative findings were disappointing. The first measure used was the total time of the psychogalvanic response. This apparently simple measure presented many difficulties. The point of departure for the measurement was the beginning of the drop (not the presentation of the stimulus), and was easy enough to determine to the nearest half-second. However, the point at which to stop the count was anything but easy to determine. The ideal situation was that in which the subject returned after the drop to the exact level of resistance at which he had been before it, but this rarely occurred. Often the resistance did not return to the previous level or returned somewhat beyond it. Often, also, secondary drops interrupted the recovery and created a further problem. The first measure, therefore, was arbitrarily taken from the beginning of the drop to the point at which the subject reached his previous resistance level or, if he failed to recover to that extent, the point of maximum recovery. Secondary drops were ignored, provided they did not completely put an end to recovery.

The results, according to this measure, are presented in Table III. It will be noted that the mean time in seconds is given for the entire list of ten words, and for the five

TABLE III

MEAN DURATION OF PGR TO WORD ASSOCIATIONS
FOR LOW AND HIGH PERSEVERATORS

Lowest 10 Per Cent	Perseveration Score	Duration of Reflex (in Seconds)		
		All words	Neutral	Emotional
1	61	40.2	41.4	39.0
2	63	37.4	39.8	35.0
3	64	32.1	47.2	17.0
4	64	19.7	23.4	16.0
5	65	22.4	21.0	23.6
6	66	22.5	26.0	19.0
7	68	24.5	21.4	27.6
8	69	20.1	18.8	21.4
9	71	--	--	--
Mean	65.7	27.4	29.9	24.8
Highest 10 Per Cent				
1	111	43.0	40.4	45.6
2	114	14.2	16.3	11.5
3	115	26.2	30.0	22.3
4	115	26.1	25.6	26.6
5	116	38.9	33.0	43.6
6	121	34.1	27.0	41.2
7	123	49.8	63.6	36.0
8	142	26.2	21.8	30.6
9	148	32.9	33.3	32.5
Mean	122.8	32.4	32.3	32.2
Difference	57.1	5.0	2.4	7.4
U*	--	22	31	20
t	--	1.347	.482	1.540
P	--	.180	.622	.154

* The U test is designed for very small samples. It has the advantage of making only one supposition: that the theoretical distribution function is continuous. For numbers over eight one converts the U to a t value, and uses Fisher's tables to obtain the probability. The formula for this conversion is given at the end of this note. The author owes his acquaintance with this test and the conversion formula to C. H. Rust, S. J., Instructor in Statistics, St. Louis University. For a description of the test, cf. Mann and Whitney (135).

$$t = \frac{U - \bar{U}}{\sigma_u}, \text{ where } \bar{U} \text{ (mean value of U) } = \frac{n \cdot m}{2}, \text{ and } \sigma_u = \sqrt{\frac{n \cdot m (n + m + 1)}{12}}$$

neutral words and the five emotion-toned words separately. The high perseverators (the highest ten per cent on the questionnaire) and the low perseverators (the lowest ten per cent) do not differ significantly whether we go by the whole list or either half of it. However, the difference on the emotion-toned words is 7.4 seconds and the level of confidence is at least 20 per cent. While this is far from significant, it suggests the possibility that differences might be significant if one could be surer of producing good psychogalvanic responses. Stimuli calculated to yield an emotional response seem suggested.

Besides the length of the entire psychogalvanic response, two other measures were tried at this stage: the length of the drop (from the beginning of the reflex to the lowest point of the drop) and the length of the recovery (counting from the lowest point of the drop to the terminus described in regard to the first measure.) Neither measure was any improvement over the first. These data are omitted for the sake of brevity.

2. The Second Stage: Galvanometric Technique

Further preliminary experimentation was begun with a view to obtaining more reliable stimuli to provoke PGR. Two things were desired: a greater chance of getting a large deflection, and freedom from disturbance of the recovery by secondary drops. Sensory stimuli were chosen because they have

proven successful in other work (128: 287) and appeared less likely than the association method to cause secondary drops due to the subject's worry about his response. It was hoped that sensory stimuli would give a sharp drop and a smooth, regular recovery so that measurements could be made with a minimum of confusion or arbitrary decisions.

Apparatus

The bridge, galvanometer, and recording apparatus were the same as before. An electrically driven timer was used to set off the stimuli at regular intervals; it also regulated the duration of the stimuli and activated the lamp which made a record of the stimulus onset and duration.

After six subjects had gone through the experiment, it became clear that better stimuli were needed. The first set of stimuli had been: 1) a bright light, 2) a bell, and 3) electric shock. For the first, an automobile lamp with reflector but without lens was suddenly shone on the face of the subject from above and at such an angle as to be startling but not painful to the eyes (the room was very dimly illuminated.) The second consisted of an ordinary Signal Junior bell. The shock was administered by means of electrodes strapped to the subject's left hand; the inductorium was set to deliver an intermittent charge of sufficient intensity to be mildly startling but not painful. The second set of stimuli consisted of 1) a buzzer that emitted a shrill, somewhat unpleasant note, 2) a buzzer emitting a note in the intermediate ranges, and 3) a Signal Junior bell. The

elimination of the light stimulus permitted the use of ordinary illumination in the room. Even when the room was darkened, a running kymograph was placed on a worktable in front of the subject so that watching the moving vane might give him something to do, and aid relaxation.

Procedure

We need mention here only those elements of the procedure that were different from those of the word-association work. A more effective screen was used. The record was taken from the right hand; the digit and ring finger were placed in the cups. The adaptation period varied somewhat with the subject but was, ideally, only five minutes. Adaptation was hastened by having the subject take one or two deep breaths and clench his fist once or twice. Besides tending to force the resistance down toward a stable level, this procedure permitted setting the shunt appropriately, provided a rough estimate of his reactivity, and to some extent prevented a difficulty previously encountered: the tendency of the subject to show no recovery at all from the first reflex or two.

The instructions were:

(Immediately after initial resistance had been read):

Place your fingers in the cups in such a way that they rest lightly, without pressing, on the edges of the cups. Keep the fingers in the cups until told to remove them at the end of the experiment. This is important to protect the instrument.

Do not move your fingers within the cups, but keep them stationary at all times.

Assume as relaxed a position as possible. Sit still, avoiding any movement of the body, head, or even the free hand. Avoid coughing, sneezing, sighing, talking, clearing the throat, or any deep breath.

There will be a wait of five minutes while I make the necessary adjustments and you become accustomed to the situation.

(Immediately before the experimental series:)

We shall now begin to make a record while you attend to the moving vane on the apparatus on the work-bench in front of you. Try to attend to it in a relaxed, dreamy, sort of way, but do not close your eyes.

Remember to avoid talking of any kind, sneezing, coughing, clearing the throat, sighing, or a deep breath. Especially remember to keep your fingers from moving. Your left hand should rest relaxed on the arm of the chair and should under no condition be moved.

The stimuli were then presented in the following order for the first six subjects: light, one-minute interval, buzzer, one-minute interval, shock; after an interval of one minute and forty seconds this series was repeated. Each series was given at least three times. It was not possible to adhere to a set number of repetitions. Some subjects would by the end of the third series show almost complete habituation to the stimuli, so that further repetition was useless; others would remain sufficiently reactive and would be given further repetitions in the hope of getting at least ten good responses, i.e., with a clear drop and a smooth recovery curve. Often the timer had to be stopped, prolonging the interval, so as to wait until the effect of some chance disturbance subsided.

For the remaining eight subjects, the series was: high-note buzzer, interval, low buzzer, interval, bell, interval, repetition of series. The duration of the stimuli was one

to two seconds. (The timer proved very unreliable.)

The subjects were 14 members of the class in Experimental Psychology II during the fall semester, 1947. They were unacquainted with the nature of the experiment.

Results

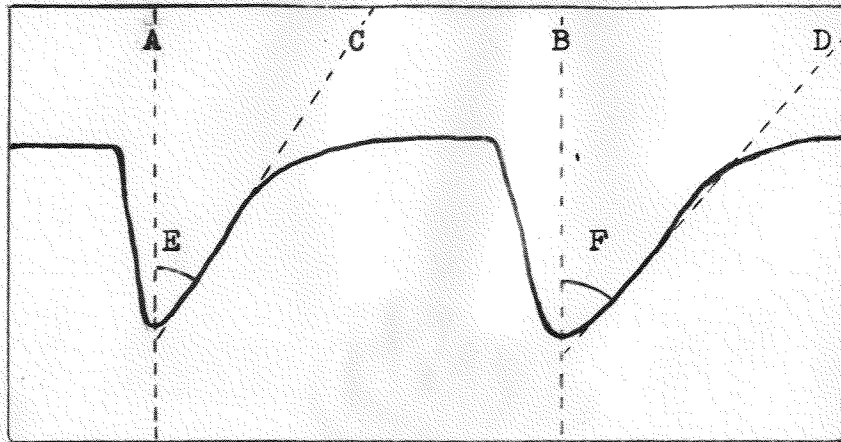
Since the main purpose of this work was to perfect the technique, we shall first turn our attention to questions of apparatus and method. The first set of stimuli had been chosen so as to provide a visual, auditory, and tactual or pain stimulus. The visual stimulus gave a good response the first time it was applied, but very little response on repetition. Responses to the electric shock were appreciable, but recovery was disturbed. For these reasons, it was decided to make the stimuli auditory. The two buzzers were added so as to get a gradation of intensity from the shrill buzzer, which was not very loud, to the medium-toned buzzer, to the bell, which was somewhat jarring. The shrill buzzer, however, gave poor results. The use of regular intervals proved a mistake. In most cases the interval was too long; in some, the stimulus was likely to come at a point where the record was disturbed by secondary or anticipatory drops. It was clear, however, that auditory stimuli were rather reliable.

In attempting to find a good quantitative measure of the speed of recovery we got the impression that the initial spurt of recovery, after cessation of the drop, was fairly

constant for each subject. Very quick-reacting subjects would show an initial recovery so rapid that the bottom of the drop would appear on the record as almost the point of a triangle; and even when the bottom of the drop appeared more rounded, after two or three seconds a definite and rapid trend of recovery would generally set in. This spurt formed practically a straight line. After this spurt, the curve would flatten out so that the latter portion of the recovery would be decidedly more gradual. With rare exceptions, secondary drops did not occur until enough of the initial spurt had taken place so that the slope of this part of the curve could be identified. Thus by drawing a straight line, to coincide with this part of the curve or to include as much of it as possible, and by dropping a perpendicular from the edge of the record through the bottom of the drop or through any part of this line, one had an angle that could easily be measured and would represent the speed of initial recovery. The process will, perhaps, be clearer if we make a diagram (Fig. 2.)

Fig. 2

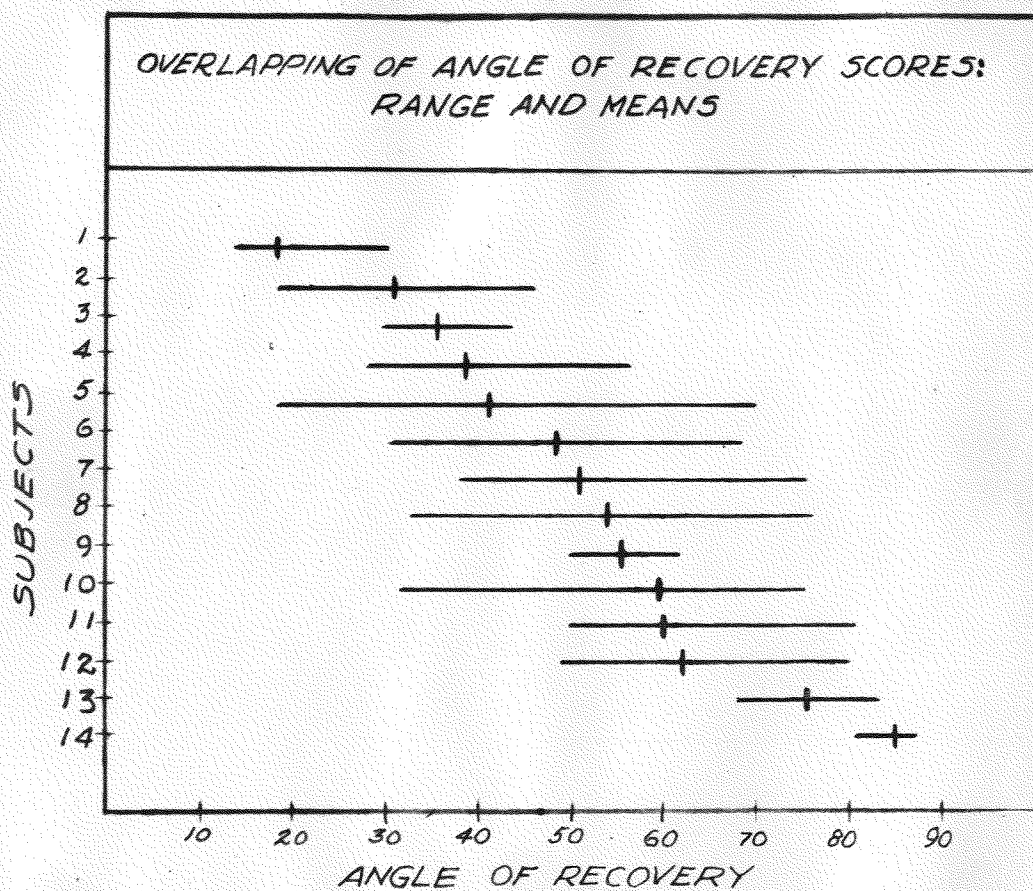
Measurement of Angle of Recovery



The solid line represents the record of the changes in resistance; the perpendiculars A and B, serve as one side of the angle; the dotted lines, C and D, are determined by the slope of the first part of the recovery curve; the angles, E and F, are the measures of recovery.

In evaluating the possibilities of this method, we may first consider the average angle for each subject and his range, so that we may compare the intraindividual with the interindividual range. It stands to reason that no measure is of value if the latter exceeds or approximately equals the former. These data are presented in Table IV and graphically in Fig. 3.

FIGURE 3



THE RANGE IS INDICATED BY THE LENGTH OF
THE LINE; THE MEAN, BY THE VERTICAL LINE.

TABLE IV

INDIVIDUAL DIFFERENCES IN ANGLE OF RECOVERY FROM PGR*

Subject	Average Angle	Range
1	19.0	14-30
2	31.0	19-46
3	35.4	30-43
4	39.0	28-56
5	41.4	19-70
6	48.5	30-68
7	50.6	38-75
8	53.6	33-76
9	55.8	50-62
10	59.8	32-75
11	60.1	50-81
12	61.8	49-80
13	75.7	68-83
14	85.3	81-87
Mean	51.21	19.0 - 85.3

* The figures given are degrees. Decimal fractions are used in place of minutes.

It may be seen that the interindividual range covers 66.3 degrees, while the maximum intraindividual range covers 51 degrees. This is far from satisfactory, but it does not completely rule out this method of measurement. The graph seems to indicate that the worst overlapping occurs in the middle of the range of scores.

Another test of the feasibility of this method of measurement is afforded by the reliability coefficient. This was

computed by correlating the mean recovery angle for the first half of each record with that for the second half. (By "half" is meant one-half the number of measurable drops.) The two distinct halves, rather than alternate drops, were taken because this procedure also provides a check on the possibility that by the end of the session the recovery angle is considerably different from what it was at the beginning. The correlation $(r)^*$ was .754, with a standard error of .277. This is not very high for good reliability. There are several possible explanations of this relatively low coefficient. In the first place, determining the one side of the angle was not always easy, since one had to fit a straight line to a curve as best he might. Secondly, an error could also be made in determining the other side, since with these records one had to drop a perpendicular from the edge of the record. Again, considerable difficulty was encountered in lining up the protractor, so that one could easily make an error of two or more degrees. Many drops which had been included should probably have been rejected because of disturbance. This method of measurement could probably have been made more reliable if these errors could have been eliminated. Since this was extremely difficult

* In this and the following chapters, all correlations derived from our data will be product-moment coefficients. When the correlation is obviously three or more times its standard error, we shall not give its level of significance.

the angle method was abandoned for subsequent work. The above coefficient of reliability, together with the data on the overlap of the measure, may be interpreted to mean that we are on the trail of a reasonably good measurement, but have not yet attained it.

Again it appeared that, whatever the difficulties of measurement, there are qualitative differences in the shape of the recovery curve for different subjects. Since there was some doubt as to the degree to which the shape of the curve was dependent on the different sensitivity of the whole circuit at different levels of subject resistance, it was necessary to choose an arrangement in which the current going through the galvanometer would be constant. This would guarantee that a drop of so many ohms would measure the same and appear the same, no matter what the level of the subject's resistance. Thus we should be able at once to compare one record with the other and make one scale which could be used with all the records. The circuit chosen will be described later.

Summary of Difficulties

It may be profitable here to review the difficulties encountered in the use of the psychogalvanometer. Secondary drops were the outstanding problem: by arresting recovery they made it difficult to get a simple measure of recovery in terms of time. The difficulty was reduced somewhat by the change to sensory stimuli, but not eliminated. Another problem was the

tendency in most subjects to drift from one resistance level to another, now up, now down; this was apparently occasioned by a stimulus in some cases, but in others not.

There were difficulties that were primarily a matter of physics. The difficulty encountered because of the varying sensitivity with the Wheatstone bridge circuit has been described sufficiently. Another important point is the period of the galvanometer. This was checked empirically, by comparison of the recovery curves when the deflection was caused by the reflex with the curve formed when a deflection of the same amplitude was produced artificially, with a resistance substituted for the subject. Another check was the comparison of drops of equal amplitude given by two different subjects. These comparisons left no room for doubt that the distinctive curve of recovery is a function of the electrical characteristics of the subject, and not of the peculiarities of the measuring circuit. In the case of extremely quick-recovering subjects, the period of the galvanometer will naturally influence the shape of the curve to some extent.

The question of the time needed for adaptation must be mentioned. It was found that about five minutes were usually enough. If the subject had not reached a stable level by that time, there was little likelihood that he would do so in any reasonable amount of time. Even when stability had apparently been reached, the beginning of the experiment proper

frequently occasioned wide fluctuations. The first and second drops were likely to be extensive, 1,000 to 2,000 ohms in some cases, with recovery nonexistent or atypically slow. The latter difficulty was removed to some extent by having the subject produce drops during the adaptation period by clenching his fist and taking a deep breath.

Longer sessions appeared to lead to somewhat unpredictable results toward the end. Best results were obtained in sessions not over a half hour. The number of stimuli could not be made standard. It was necessary to get a satisfactory number of responses so as to have reasonably reliable data. In so many cases was the recovery from a very good drop spoiled by secondaries that one had to keep on trying. In other cases, the effectiveness of a stimulus was exhausted by the second time it was used. In still other cases, no stimulus would work after a certain time. The order of presentation could be standardized only after a fashion. Besides the need of omitting stimuli that had already lost their effect, the uniformity would be only illusory: many of the drops would subsequently be eliminated because of disturbances, so that the order of the stimuli for the drops actually used in the measurement would not be the same from one subject to the other.

C. TENTATIVE CONCLUSIONS

Before going on to the description of the main experiment, it will be profitable to summarize the conclusions

reached so far.

1. Individuals seem to differ in the characteristic shape of the curve formed by the record of the reflex drop and the recovery. It is not very difficult to determine roughly the typical pattern for each individual if the drops are appreciable.*

2. A simple count of the number of seconds required for recovery is not a feasible measure of speed of recovery because secondary drops are too frequent to permit the counting of more than a fraction of the drops in most cases. When this measure was used, despite this difficulty, it failed to distinguish between the high and low perseverators.

3. A measurement of the angle of the ascending recovery curve showed some promise, but did not prove sufficiently reliable. It was subject to appreciable overlap of the single scores of one individual with those of the next. It seemed, however, that we had reasonable hope of finally obtaining a fairly reliable measure of rate of recovery based on the first part of the recovery curve. To be on the safe side, very small drops must be eliminated from the computations, since they tend to be slower than more ample drops from the same subject.

* As we shall subsequently see, this first conclusion has to be modified considerably.

CHAPTER VII

THE MAIN EXPERIMENT AND ITS RESULTS

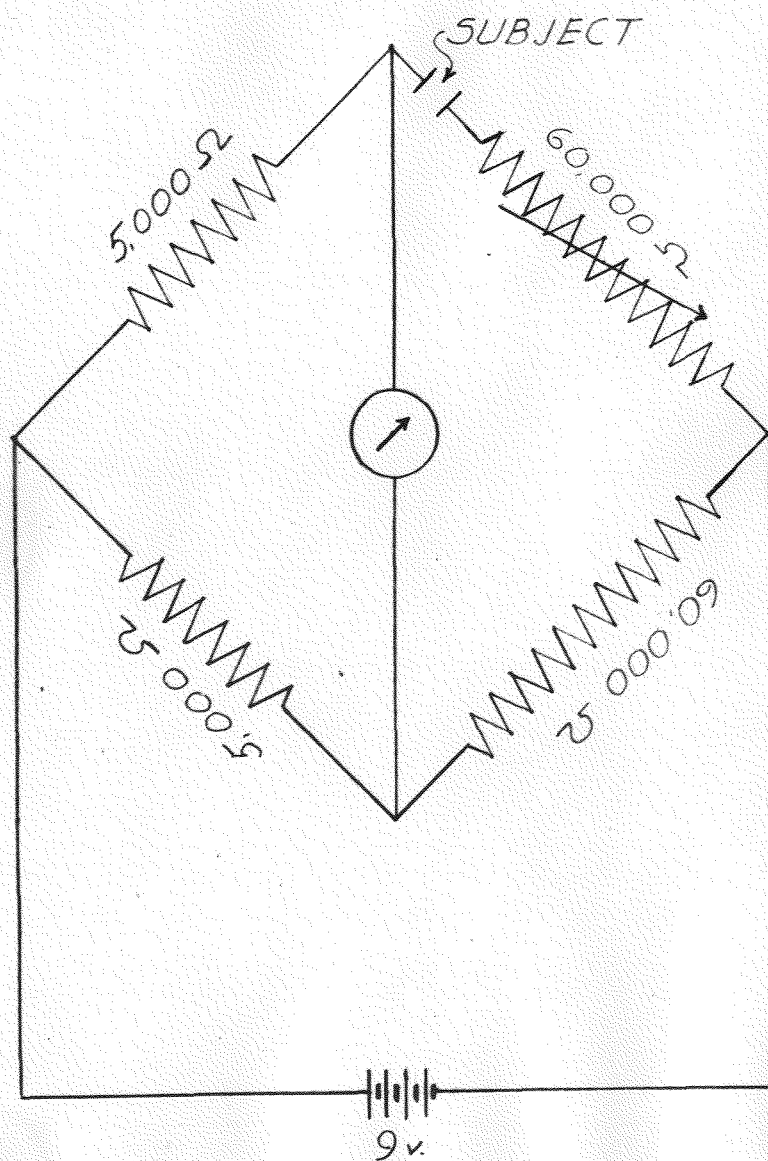
A. DESCRIPTION

In discussing the experiment proper, it will be helpful to recall that we intended to measure the introspected perseveration of a group of subjects and attempt to correlate it with a measure of the duration of response, or, conversely, the rate of recovery. With the improvement of the apparatus and technique, this investigation seemed feasible. We shall describe the PGR work first.

Apparatus

Again we shall mention only those features of the apparatus which had been changed. The bridge circuit now used was similar to that described by Woodworth (128: 278). However, as will be noticed in the schematic diagram presented in Figure 4, the pairs of resistors which fan out from the battery leads are balanced instead of being of different resistance. The resistances on the one side are somewhat higher than those in Woodworth's circuit. This reduces the danger to the galvanometer if the subject suddenly removes his fingers from the cups; it also reduces sensitivity.

FIGURE 4
GALVANOMETER CIRCUIT



Having the subject in series with the adjustable resistor makes it possible to keep the same total resistance in the bridge despite the different resistance levels of the various subjects. As a consequence, the current through the galvanometer remains constant, as does also the voltage at this point; except, of course, for the change induced by the reflex itself. The voltage and current will also be slightly different if the galvanometer is not kept quite at balance; the comparative difference will, however, be slight because of the high resistances used in our circuit.

There is some error in our resistance box. The variable resistor consists of two sections with steps of ten thousand and one thousand ohms respectively. The average error in the former is 0.06 per cent; in the latter, 0.34 per cent; the total error is well within the range of laboratory bridges and affects only the measurement of resistance level, since the ohm value of a given deflection is constant in this circuit.

The correctness of the latter claim was tested by recording a drop of one thousand ohms. The reading was exactly the same, 34 mm., at the twenty-and thirty-thousand levels; slightly different, 33.5 mm., at the forty-thousand level. This difference is partially due to the distortion introduced by recording an angular deflection on a flat surface. At the first two levels the galvanometer swung from very near the point of balance to one extreme of its range; at the other level it swung between points about equidistant

from the balance point on either side. Though it would have been possible to correct for this distortion, it was not deemed practicable in view of the relatively slight error involved.

A similar check was made in order to satisfy ourselves that the excursion of the galvanometer progressed linearly throughout its range as the resistance change progressed. Aside from the difficulty just mentioned, the record of successive increases of resistance change showed that linearity of measurement had been attained.

The battery potential used was nine volts. This relatively high voltage was necessary to get reasonable sensitivity. The potential across the subject electrodes varied with the subject's resistance; at 10,000 ohms it was 1.38 volts; at 20,000 ohms, 2.77 volts; at 30,000 ohms, 4.15 volts; at 40,000 ohms, 5.53 volts. With low-resistant subjects, who are more sensitive to electric shock, the voltage is low; nevertheless, one or two complained that they sometimes felt a shock. According to Thouless (123) an external potential of one volt at the subject electrodes renders the Tarchanoff effect negligible. Though he advises the use of two volts for safety, this could not conveniently be done.

A change was also made in the recording apparatus. Seconds were now indicated by a flashing light also mounted in the galvanometer box; this light produced a fine line across the record, perpendicular to its edge. The galvanometer light

proper was now continuous, as in the usual arrangement.

An improved timer was employed. Though it was designed to deliver a stimulus at constant intervals, it was used solely to provide a contact of uniform duration and hence a uniform duration of stimulus. It could be turned off and on by a silent push-button switch. After experiencing some difficulty because the first two subjects were alert enough to notice that the motor had been started (though the noise was really very slight), the experimenter was able to control the motor so that it started up only a second or two before the stimulus. Subsequently, only one subject mentioned the noise. A relay activated the stimulus-indicating lamp; its noise was masked by the stimulus.

There were three electrically operated stimuli: a buzzer producing a tone in the middle ranges and of moderate intensity, a Signal Junior bell, and an old, raucous automobile horn. By means of a switch, combinations of these stimuli could also be used. The buzzer was bolted to a sort of metal drum, to amplify its intensity, and hung beneath the subject's chair; the bell was near the wall in the back of the subject and about four feet to the subject's right; the horn, near the same wall but only two feet to his right. With the very first subject it became apparent that other stimuli were also needed if enough usable reflex drops were to be obtained. These were at first chosen extempore, but were then somewhat uniform, de-

pending on the reactivity of the subject. These added stimuli were: dropping a cigar box, clapping the hands before the subject, dropping a heavy board. On occasion, further stimuli had to be improvised, as, for example, when the experimenter, in the vain hope of getting an appreciable reaction out of an almost completely nonreactive subject, threw a small salve jar to the floor with such violence that the pieces almost struck him in the face.

Procedure

There were some differences of procedure in this part of the experiment. The subject was asked to wash his hands with soap and warm water. This proved a definite advantage in that usually it lowered the level of resistance and hence gave greater assurance that the level after the five-minute adaptation period would be reasonably constant; in some cases, however, the resistance mounted during this period. The practice of asking the subject to take a deep breath and clench his fist was retained. The kymograph was discarded as unnecessary. The intervals between stimuli were variable. The general principle was to wait until the resistance had gone back to a relatively stable level. In some cases the intervals were long; in others they were rather short. Some subjects, who appeared to have become adapted to a certain stimulus, could be aroused by this stimulus if the interval was prolonged. The number of stimulations varied according to

the speed with which the subject adapted to one kind of stimulus and according to the number of drops that had been satisfactory. In many cases the experiment had to be discontinued because the subject no longer reacted appreciably to anything. The aim of the experimenter was to get ten good drops.

The order of presentation was variable, depending on the subject. Whenever possible the following order was kept: the buzzer was rung five times, then the bell five times, and the horn five times, then the bell and horn together once or twice; then came the dropping of the box, a handclap, the board, another handclap, another board, and finally the bell and horn.

The instructions were the same as those for the last preliminary experiment except for the omission of the reference to the kymograph and the slight changes of wording thus necessitated. Before starting the experimental run, the subject was invited to change to a more comfortable position if he wished; in some cases he was advised that his position (for example, the legs crossed) might cause trouble in the form of discomfort or involuntary movements of the free leg.

Resistance was recorded as soon as the bridge could be balanced and after the five-minute adaptation period; it was also recorded at each new adjustment after that. An effort was made to keep the time of the experiment, from insertion of the fingers into the electrolyte to the end of the recording, as close as possible to 15 minutes. The majority of the subjects took 13 to 20 minutes; the minimum was 10 and the maximum, 32. The

prolongation in this last instance was due to disturbances caused by noises outside the room and to the great frequency of very extensive drops followed by a slow recovery.

Subjects

The subjects were 32 members of a class in experimental psychology, and 20 members of a class in the psychology of learning. Two had participated in the second preliminary experiment. The subjects ranged in age from 19-2 to 31-7, with the mean at 23-0 and the standard deviation at 2-4. Notes were taken on the general health and fatigue condition of the subject as well as anticipation or fear, the time of beginning, and end, the temperature, external noises and disturbances, and whatever remarks the subject wishes to make regarding his reactions. The sessions were conducted in the early morning and the early afternoon. The temperature ranged from 66 to 74 degrees. The experiments ran from March 2 to March 30, 1948.

PGR Records

The photographic records were cut and pasted on cards of convenient size so that they could be studied by the judges who were later asked to sort them into qualitative categories. After eliminating some drops, for reasons to be explained later, the satisfactory drops were microfilmed, so that the records could be amplified by using a microfilm reader. The amplification thus obtained amounted to 4.4 times the original, so that the accuracy of measurement was presumably increased by that

amount. It was, however, necessary to measure some of the drops directly from the original records.

Questionnaire

The questionnaire was administered by the experimenter to each of the two groups of subjects separately. For one group, this was 27 days after the last one of them had gone through the PGR experiment; for the other, it was 14 days. Four subjects had to be given the questionnaire individually. The questionnaire was scored only for the 26 items retained as discriminatory.

Experiment on Day-to-Day Variability

It was necessary, in view of the inadequacy of the existing evidence in this matter, to have some data on the constancy of the recovery rate from day to day, since it could have no consistent relationship with perseveration if it were itself quite variable. Hence the following check on this point.

Two subjects participated in the second preliminary and in the final experiment; hence their recovery scores could be compared. The intervals were three and five months respectively. For a more intensive study, the experimenter and two other subjects were tested again at intervals. These subjects we shall designate as S1, S2, and S3, calling the other two S4 and S5. S1 was 46-11 at the time of the experiments; he was regarded by the experimenter, on the basis of

his first record, as a moderately fast recoverer (Recovery. Category II^{*}); he was thoroughly acquainted with the galvanometer.

S2 was 35-9 at the beginning of the experiments; he was regarded as having a very fast recovery rate (Category I); he was also thoroughly acquainted with the procedure. S3 was 35-5 at the beginning of the sessions, was taken as a very slow and incomplete recoverer (Category IV), and, though not naïve, was not well acquainted with the procedure. S4 was 20-0 at the time of the second testing, and provided another case of very fast recovery. S5 was 30-7 at this time and was thought a moderately slow recovered (Category III). As the reader may have surmised, we wished to have at least one sample of each broad category of recovery. All the subjects were in fair health at the time of the experiments.

The time intervals were determined partly by design, partly by circumstances. S1 had his first session on June 15, 1948, after which the intervals were 1, 2, 2, and 4 days. S2 began June 18, and the intervals were 2, 4, 24, and 4 days. S3 began June 20, and repeated at intervals of 1, 2, 11, and 14 days. All the sessions, with the exception of the first one for S3, were in the morning, though the exact time varied within a range of two hours. The sessions lasted about 15 or 20 minutes.

The stimuli had to be varied because of adaptation,

* These categories, which were used in the qualitative analysis of the records, will be explained later.

but an effort was made to keep them similar to those used with the main group and, when possible, the same. With S3 quite a variety had to be used; for him only, word association and other verbal stimuli were tried. The rest of his stimuli were as purely sensory as those for the main group; for example, a high metal stool was pushed over so as to fall to the floor with a bang, a pile of wood and iron was made to fall in an adjoining room, and so on. Actually only three of the successful stimuli for him were other than sensory. The other stimuli were: the buzzer, the electric bell, the horn, dropping a box, a board, clapping the hands, dropping a piece of heavy metal, a gong, two types of whistle, bursting a toy balloon, firing a cap-gun, suddenly turning up a radio from silence to full blast turning on a noisy motor, breaking a jar on the floor, slamming a hammer down on the table. It will be seen that these are either the same as those used with the main group or very similar. Over and above these, one subject was threatened with a hot soldering-iron, which was brought so close to his hand that he could feel the heat; however, the drops were so extensive that the galvanometer went off the record and hence they could not be measured.

The procedure in these experiments was the same as that for the larger group, except that it was not necessary to repeat the instructions. The complete instructions were used at the first session only with S3, who was not thoroughly fam-

iliar with galvanometer work; for the other two subjects and for repeated sessions they were replaced by a brief reminder of the need for silence and avoidance of motion. There were no changes in apparatus.

Difficulties

Despite the improvements in apparatus and procedure, there were still difficulties in getting galvanometer records free of the effect of extraneous disturbances. It soon became apparent that some subjects would give quite good records; i.e., would react sufficiently to the stimuli and in most cases return smoothly, if not quickly, to a stable level; while others simply would not do so. The first one or two reflex drops were most likely to show a poor return, though results were better than they had been in the preliminary work. All stimuli showed the effect of adaptation with almost all the subjects, though the rate and degree of adaptation varied from subject to subject: some would give little reaction to even the second application of the same stimulus, while some showed a very slow adaptation. The mechanically controlled stimuli, i.e., the buzzer, bell, and horn, proved inferior for our purposes to the sudden and poorly controlled stimuli, i.e., the handclap, dropping of box, etc. The latter stimuli were more effective in producing a reaction, and this reaction tended to run its course smoothly, so that the curve of recovery was generally smooth and quite consistent from drop to drop. This may have been due to the fact that these stimuli occurred at the end of the experiment, after the subject

had ceased worrying about what was to happen or perhaps had become better adjusted physiologically to the situation. The fact that there was such consistency between each subject's responses to these stimuli affords sufficient reason for concluding that the difference in intensity or duration between the various presentations of these stimuli had little or no effect on the results. Though it was impossible to make successive handclaps, for example, of equal intensity; this did not seem to alter the shape of the curve appreciably.

A further difficulty, which seems inherent in the fluctuating nature of the resistance level, lay in the frequent occurrence of what we have called drifts of resistance up or down. They were rather unpredictable. Rarely did the subject maintain a constant level throughout. We shall return to this question later, when we discuss the quantitative measure.

Qualitative Analysis

When we speak of qualitative analysis we mean the judgment of the speed and type of recovery by inspection of the shape of the whole curve. Some subjects consistently recover from the reflex more quickly than others; this can be seen at once by the sharp angle at which the curve of recovery swings upward after the bottom of the drop has been reached. These subjects are also quick reactors, and this can be seen by the sharp angle of the downward sweep as the drop is in progress.

Those who recover more slowly give a wide-sweeping curve in which the angle of ascent is more obtuse; the downward sweep of the drop is also less sharp, with one exception in our records. This qualitative difference will best be seen, without further description, from the portions of actual records which we shall reproduce later. The records that lie at the extremes of speed or slowness are relatively easy to distinguish, and there is rather high consistency from one drop to another. Within the intervening ranges, discrimination is somewhat more difficult, and there is not always the same degree of consistency.

In determining the number of categories into which to classify the records, various factors might have been considered. Speed of recovery is closely associated in all but one of the records with speed of reaction. Secondary drops are perhaps a factor of nonrecovery, so that we might be justified in considering the presence of a number of secondaries as being evidence of slow recovery. Completeness of recovery is another factor; i. e., the extent to which the subject regained his previous level of resistance or failed to do so. It was decided to give the principal attention to the speed of recovery, including the speed of the drop as an associated phenomenon. Since notable failure to recover was a prominent characteristic of some of the slowest recoverers and was not found in the other records, this characteristic was made the basis for a fourth category. Hence we selected four categories: I: Very fast recovery, II: Moder-

ately fast recovery, III: Slow recovery, IV: Slow and incomplete recovery. The reasoning behind placing the last category along the same dimension as the rest is that the members of this last group are so slow to recover that they would take an indefinite time to do so. Further description of the categories will be found in the instructions for the judges who were asked to classify the records.

There were six judges: the experimenter and his director, one experienced clinician and instructor in psychology who had had some experience with the galvanometer, two other experienced clinicians and instructors in psychology who had some acquaintance with this sort of work, and one graduate fellow who was rather unacquainted with galvanometric techniques. These judges were asked to sort the records into four categories, and were supplied with the following instructions, together with four sample records, one for each category. The experimenter, of course, did not have the samples, since these were chosen by him from his own classification; nor did he have the instructions before him, the latter serving as a digest of the principles he had used in making the classification. The director had the samples, with brief oral instructions as to the categories. The instructions for the other judges are reproduced here in full:

General Directions

Please place these records in four categories as described below.

Classify them according to the descriptions and samples without concern about the number in each cate-

gory.

It will often be necessary to judge according to the majority of the drops, since there will not be complete consistency from drop to drop. Some of the drops are marked "No". These will, for various reasons, be eliminated from the quantitative computations. In the present classification, they should not be eliminated unless they represent a slight drop in comparison with the rest.

Only drops occasioned by the stimulus should be considered. These can be identified by the fact that a number is written above them thus: 5.

The vertical lines across the record are time lines, indicating seconds.

Speed of recovery, as understood in the descriptions below, means the relative time between the end of the drop and the point where the subject has gone back to his previous level, or at least about as far as he is going to in the particular instance. Usually it will be judged by the angle or curve of ascent.

Speed of drop is similarly indicated by the angle of the descending line of the curve.

As a general rule, the drops from #11 on will be the clearest and easiest to judge. These should be taken as the deciding factor when in doubt, provided there are enough in the particular record in question.

Secondary drops (i. e., drops occurring within 20 seconds after the line reaches its lowest point) will often create a problem. When it is clear from the rest of the record that they are consistently interfering with recovery, they should be taken as a manifestation of poor or slow recovery.

Note on procedure:

If you find it difficult to classify immediately into four categories, it may help to begin by making two groups: Fast reaction and recovery (Categories I and II), and Slow reaction and recovery (Category III and Category IV). Then break these up into two.

Description of Categories

I (Indicating the sample.)

1. Return is sharp and rather complete.
2. Drop is also sharp.
3. The bottom of the drop shows a rather sharp point. It is almost the point of an angle.

II (Indicating the sample.)

1. Return is fairly sharp, but less sharp than in I. It will be rather complete - about as complete as in I.

2. Drop is almost as sharp as in I.
3. The bottom of the drop rounds out a bit. But it would still be possible to form a reasonably good angle by projecting the ascending and descending lines down a bit.

III. (Indicating the sample.)

1. Return is slow, but fairly complete.
2. Drop is also slow.
3. Bottom of drop is well rounded. It is better described as the arc of a circle than as an angle.

IV. (Indicating the sample.)

1. Return is not only slow, but also incomplete. The incompleteness is the feature distinguishing this category from Category III.
2. Drop is slow (generally).
3. Bottom of curve is rounded (generally), but not necessarily more rounded than in Category III. However, it will generally present a more "open" appearance.

N.B.: The model for this category represents an extreme case of poor total recovery, and hence need only be approached in the other records if you wish to put them in the same category.

It presents a difficulty in that the drop is fairly rapid and the initial recovery is in some instances fairly quick. The important feature is that the recovery, taken as a whole, is more often very slight and very slow.

The experimenter was present at the beginning of each judge's work to explain further and answer questions, but then left so as to avoid embarrassment, or dropping hints, or acting as a court of appeal. One record showed only three very minute drops with no recovery whatsoever. Two judges put this aside. They were subsequently asked what place they would assign it if they had to put it in one of the categories. One immediately assigned it to the fourth; the other required further questioning, which the experimenter tried to make nonleading, before she also placed it in this category. The sample for Category IV

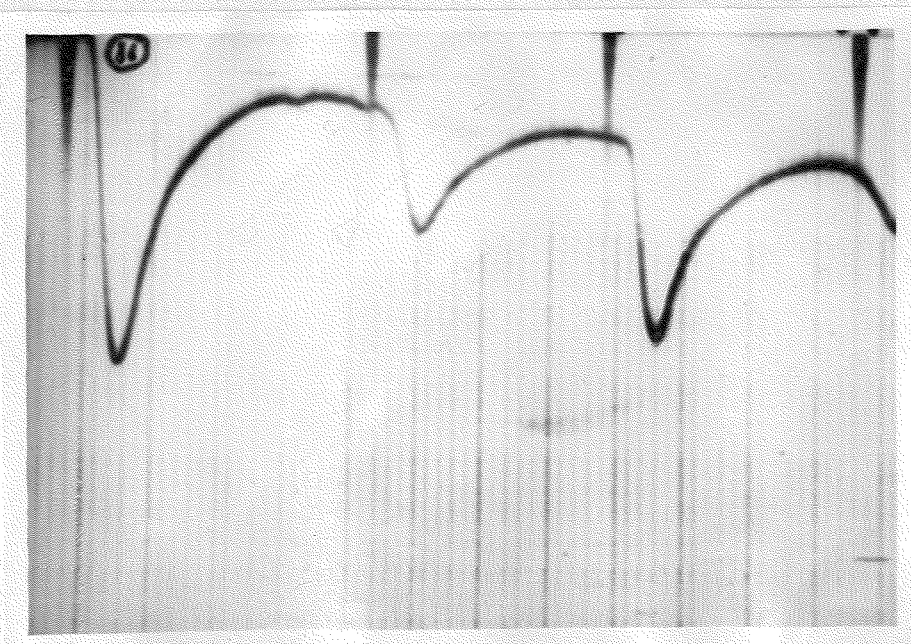
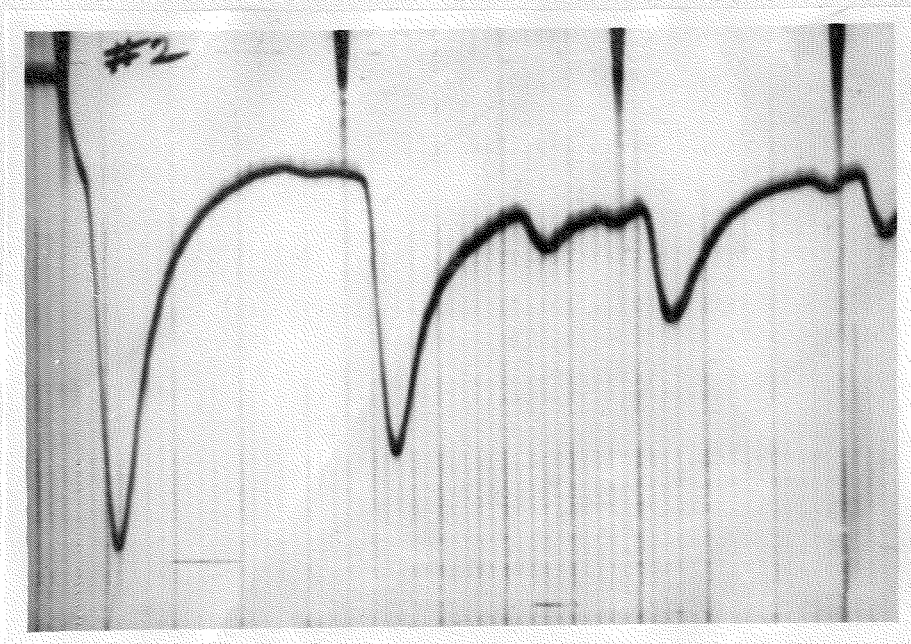
caused difficulty; nevertheless, it was used because it illustrated the incomplete recovery better than any other. Selections from the four samples are reproduced in Fig. 5.

Aside from the four sample records, which are excluded in all the following estimates of reliability, there was complete agreement of all six judges on only 11 out of the 48 records; at least five out of six judges agreed on 22 of the records, while at least four out of six agreed on 36 of them. For a more accurate determination of the agreement between judges two methods were used: first, the percentage of agreement was calculated for each judge in relation to every other judge and, secondly, correlations were computed between each judge's classification and the composite rating or score obtained from the pooled classifications. These data are presented in Table V. The percentages represent those records in regard to which the two judges in question agreed; hence no account is taken of whether, when they disagreed, the respective categories assigned a given record differed by one or two steps. The correlations were done by first pooling the ratings of the six judges, putting the data on the normal curve, and running the product-moment correlations with the same normal-scale values for each coordinate. Thus, Category I was used 43 times; Category II, 108 times; Category III, 91 times; and Category IV, 46 times; the grand total was 288, which is the number of records, 48, times 6, the number of judges. The resultant

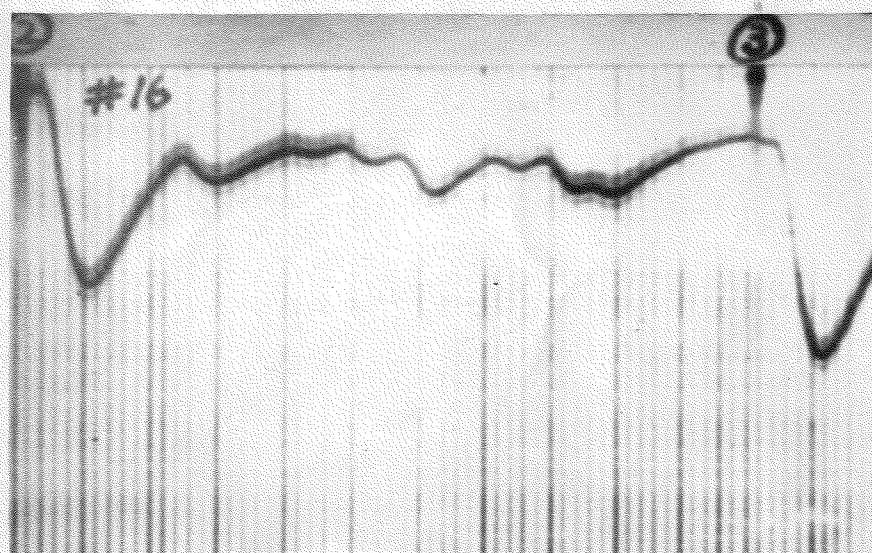
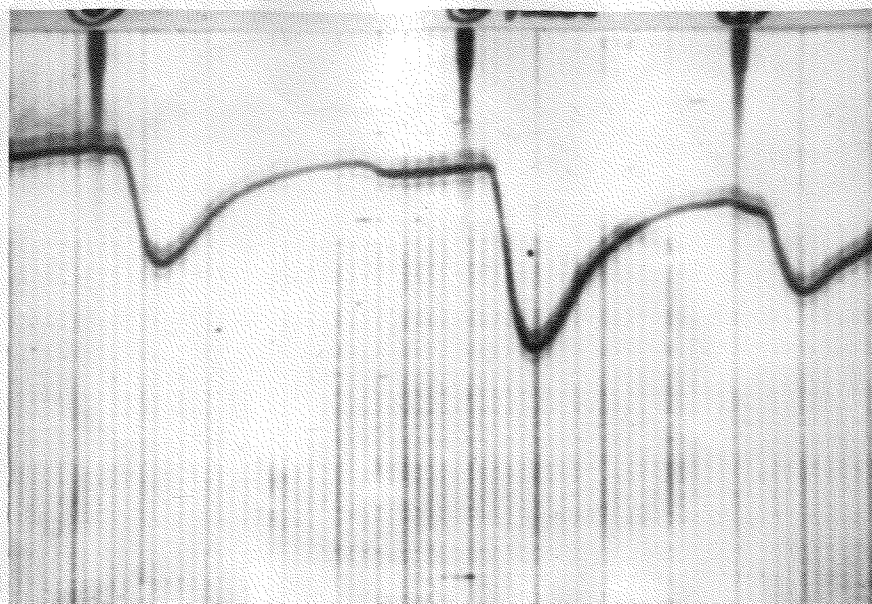
Fig. 5

CATEGORY I: VERY FAST RECOVERY

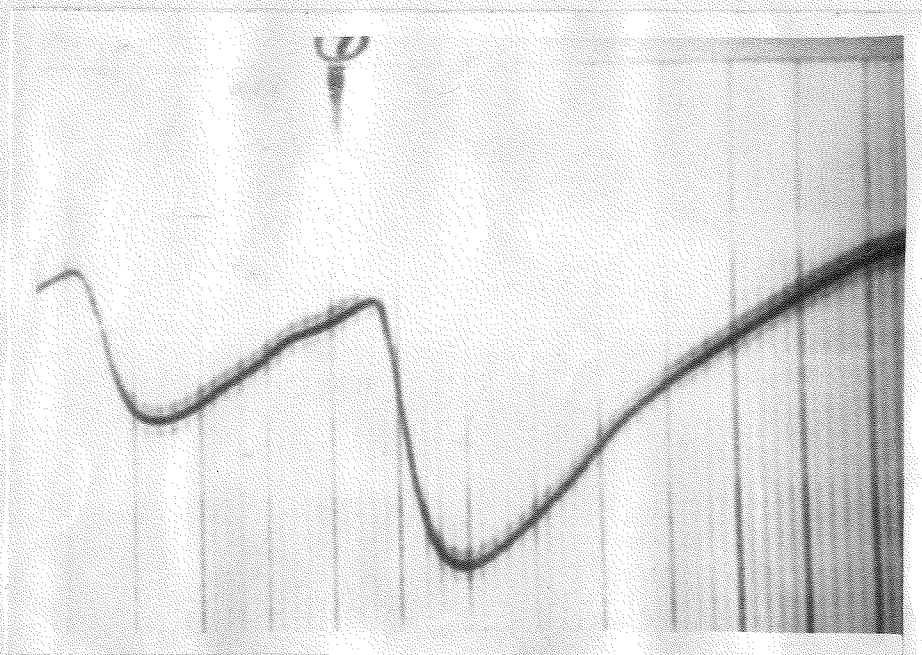
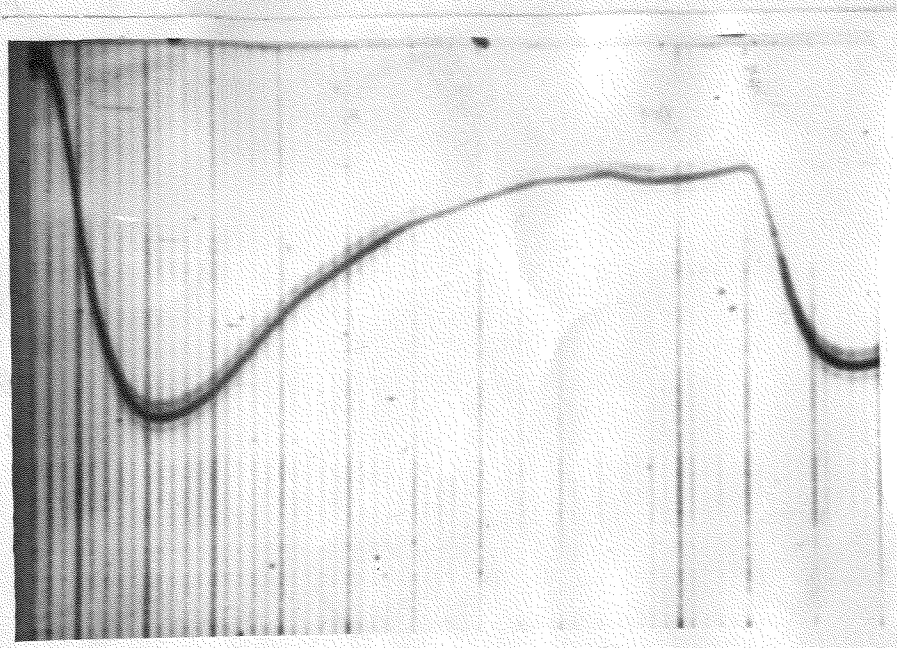
(Vertical lines indicate seconds. The black band at the top indicates the stimulus.)



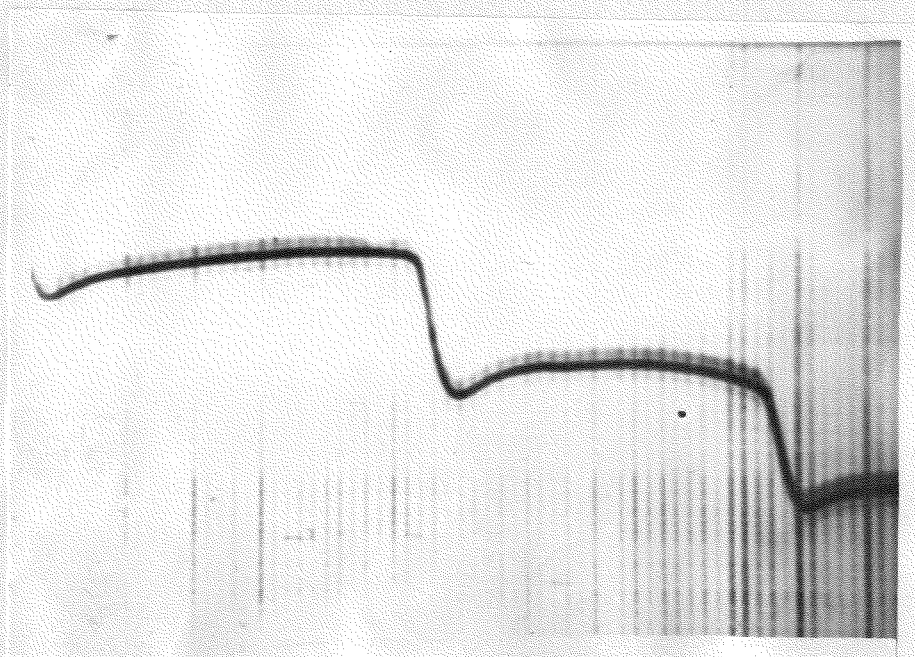
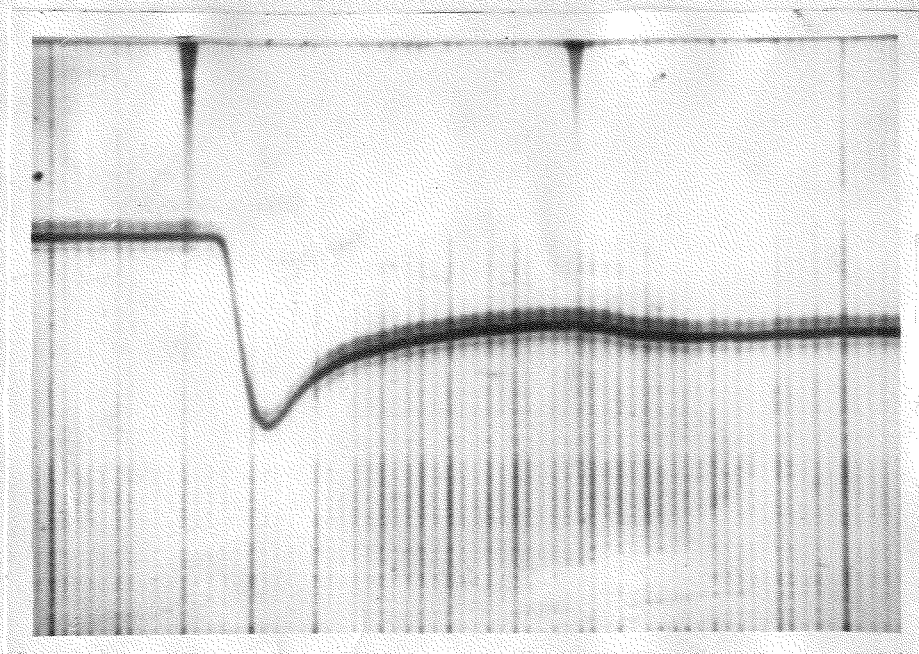
CATEGORY II: MODERATELY FAST RECOVERY



CATEGORY III: SLOW RECOVERY



CATEGORY IV: SLOW AND INCOMPLETE RECOVERY



scale values were: Category I, -1.56; Category II, -.44; Category III, +.49; Category IV, +1.52. The composite score for each subject was then obtained by converting the category value to the normal-scale value and computing the mean of the six values assigned him by the six judges respectively. With the category values thus converted to normal scale values, the correlations were then computed according to the usual formula.

TABLE V

AGREEMENT OF SIX JUDGES IN RATING SPEED OF RECOVERY FROM PGR

A. Per Cent Agreement

Judge	1	2	3	4	5	6	Mean
1	--	60.4	52.1	64.6	58.3	54.2	57.9
2		--	66.7	62.5	60.4	70.8	64.2
3			--	54.2	56.3	62.5	58.4
4				--	70.8	47.9	60.0
5					--	66.7	62.5
6						--	60.4

B. Correlations with Composite Scores

Judge	<u>r</u>	Sigma
1	.899	.145
2	.937	.145
3	.890	.145
4	.841	.145
5	.879	.145
6	.883	.145

The percentages of agreement are much greater than mere chance expectancy, which would be 25 per cent, since there are four categories and each subject would, by chance alone, have an equal opportunity of being assigned to each category. The correlations are obviously significant and are reasonably good for such work. No attempt was made to use the correction for broad categories because the experimenter felt that, in view of the percentages, such a correction would represent the agreement as better than it actually is. It is true that we should like to see all the correlations at least in the nineties when we are estimating the reliability of any measure. It appears, however, that we can claim reasonably good reliability for this qualitative method of treating the records.

Correlation of the Composite Scores with Perseveration

These composite scores, derived from the judges' qualitative classification of the records, correlated $-.346$ with the perseveration scores obtained on the questionnaire. The standard error was $.140$. The correlation is significant at the 2 per cent level and almost at the 1 per cent level.

A word about the sign of the correlation and the method of computation. The normal-scale values were used for the qualitative categories. The sign came out plus because of the way we had numbered the categories and computed the normal-scale values. We have called it minus --in effect reversing the scale-- for convenience of interpretation and comparison with the quantita-

tive scores. This introduces no error, since the same value will be derived either way. Ease of interpretation demands that some change be made, since the per cent recovery scores go from slow to fast recovery, whereas the categories are numbered from fast to slow. Hence the meaning of the obtained correlation would be that low perseverators tend to be slow recoverers. The tendency is very slight and of uncertain statistical significance.

Besides correlation, another method was also used to estimate the relationship between these two measures: that is, the comparison of the mean perseveration scores of the groups placed in the four categories of recovery. Among several possibilities, we have selected the following: 1) using only those subjects on whom all the judges agreed, 2) using those on whom at least five out of six agreed, and 3) using those on whom at least four out of six agreed, and 4) using all the subjects, but determining their categories from the composite normal-scale scores. This last demands explanation. The normal-scale values for each category are its mean distance on the abscissa from zero, which is the mean of the distribution under the normal curve. In calculating this mean, one first has to get the two limits of this area along the base-line of the normal curve, in terms of sigma distance from the mean of the distribution. Two adjacent categories have a common limit, and the base-line value (in terms of x/σ) is the normal-scale value of this limiting point. We can, therefore, take our composite normal-scale score for each subject and assign him to his category, from I to IV,

according to the limits within which he falls. The outside limits for categories I and IV are arbitrarily taken as minus and plus 3.09 respectively, since only 0.1 per cent of the area of the normal curve would lie beyond these points under either extreme of the curve. The limits of the four categories are presented in the following table.

TABLE VI

CATEGORY LIMITS

Category I:	-3.09	to	-1.04
Category II:	-1.04	to	-0.06
Category III:	-0.06	to	0.99
Category IV:	0.99	to	3.09

A subject having a composite score of $+ .83$, is placed in Category III: another, with a score of $- .29$, is placed in Category II; and so on.

The comparison of the mean perseveration scores of the groups, according to the four methods of grouping is facilitated by Table VII. Upon inspection of the data for these various groupings, two things stand out: that there is evidence that the lower and upper extremes are differentiated significantly, and that in the middle ranges there is little differentiation. Those who show the fastest recovery tend to have low perseveration

scores, while those who show the slowest recovery tend to have high perseveration scores. The range of scores indicates that this tendency is subject to quite decided exceptions, with the scatter in the middle categories (II and III) most noticeable.

What is to be made of the evidence of a significant relationship between perseveration score and the two extremes of recovery rate, whereas the correlation was not significant? The answer may be that in either or both calculations there are errors due to broad grouping or the roughness of the measures. Both these factors could influence the computations sufficiently to yield now a significant, now a nonsignificant statistic. One thing seems to be clear, that the relationship between the two variables in the middle ranges is not very close. We do not, however, wish to belabor this matter, since the evidence in this case is rendered of little value by the results of the quantitative analysis and especially of the check on day-to-day variability.

TABLE VII

MEAN PERSEVERATION SCORES FOR THE FOUR RECOVERY CATEGORIES

Category	N	Perseveration	Range	Diff. (IV-I)	U	P
All judges agree:						
I	3	41.0	36-44			
II	4	52.5	45-60			
III	2	55.5	49-62			
IV	2	62.5	59-66			
5/6 agree:						
I	4	47.8	36-68			
II	10	53.1	42-64			
III	4	53.0	46-62			
IV	4	56.3	49-66			
4/6 agree:						
I	7	46.1	33-68			
II	11	53.9	42-64			
III	12	51.7	26-71	16.9	4	.007
IV	6	63.0	49-79			
By x/σ limits:*						
I	8	45.3	33-68			
II	14	53.6	38-64			
III	22	54.9	26-81	17.5	8	.005
IV	8	62.8	49-79			

* For the meaning of this grouping see the immediately preceding text.

Quantitative Results

It had previously been determined to use some measure of per cent of recovery after a certain time, counting from the end of the drop in resistance. A preliminary trial with a sample from the upper and lower ends of the distribution for perseveration showed that five seconds was a convenient time to adopt, since it allowed one to make a measure at a point where the curve of recovery was definitely in progress and sufficiently characteristic of the individual's trend of recovery. In only a few instances was the recovery so slight as to cast doubt on the validity of the measure for the subject in question. A period of ten seconds proved to be too long, since too many of the drops were spoiled by supervening secondary drops.

In the case of two subjects, the measure had to be abandoned and their records excluded from the following computations. The one was an instance in which there were only two measurable drops in 21 stimulations (the starting-point of a third drop could not be determined), with each drop measuring only 44 ohms and showing no recovery at all. This subject, it was later found, was extremely hard of hearing in one ear, but had not himself discovered this until well after the experiment. The other subject gave good drops, but the experimenter could not decide how to use the data; the predetermined method of measurement yielded a minus recovery percentage, which was certainly not representative of the actual fact. This subject had made a determined

effort to relax by fixating on a point on the wall before him. Though he could not inhibit the reflex, his resistance constantly mounted during the experiment at a rather uniform and rapid rate. The precise nature of the difficulty of measurement here will be clear from the following general explanation of the method of measuring those drops in which the resistance had been drifting up or down before the stimulus.

When the subject maintains a constant level of resistance, so that before the drop the line described by the galvanometer is straight and parallel to the edge of the record and after the drop it resumes a straight and parallel line, the ordinary procedure is to measure from this line. The assumption is that the subject would have continued this straight line had the stimulus not been given. When, however, the subject's resistance is mounting or sinking at a constant rate, it is reasonable to assume that he would have maintained this tendency at least for an appreciable time, and that the measurement of any drop that may occur as a result of stimulation should be taken from the point where his resistance would have been had no stimulus been applied. This assumption necessitates determining the line of drift and projecting it an inch or more to the right. From this slanting line, we drop a perpendicular and measure to the bottom of the drop and similarly to the point which the curve has reached after five seconds. This was done whenever it appeared called for and whenever it was possible to determine the line of

drift with a reasonable degree of accuracy. In doubtful cases, in which it was not clear whether there was really a drift or not, both the ordinary and the special measurement were taken, and that one accepted which conformed better to the rest of the subject's drops. However, when the drift was clear, this special measurement was used, no matter what the results, since the experimenter was convinced that it fitted the facts better.

In some cases, a consistently slowly-recovering subject had not completed his recovery when the next stimulus was applied. This may have been due to a mistake in judgment on the part of the experimenter, but he had feared that waiting further would unduly prolong the experiment and introduce disturbing factors which would be as serious a source of error as the failure to wait out the recovery. An effort was made to keep the total time of the experiment as constant as possible for all subjects. In measuring the drop and recovery in these instances, a choice had to be made between the ordinary and the special method. The former was used whenever the subject seemed, upon inspection of the record, to be maintaining a fairly constant general level of resistance.

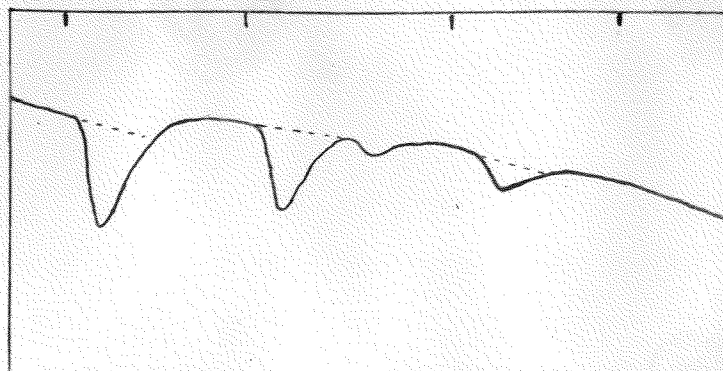
Incidentally, this whole discussion illustrates the difficulties encountered in this type of work. One has often to take his choice between assumptions like the above or simply rejecting the record. In general, our policy was to reject single drops whenever there was any doubt about the measurement

and it could not be resolved in any manner that seemed reasonably objective. Thus, for another example, when the line of drift was so irregular that it could not be determined with any assurance, the drop was rejected. Some subjects, however, were so unstable electrically that an irregular record and hence variability of measurement seemed entirely characteristic of their performance. The various types referred to may be illustrated by the tracings of sample drops presented in Fig. 6.

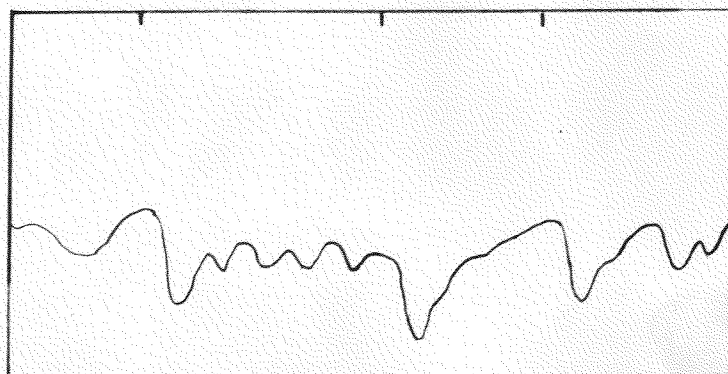
Though the experimenter desired to get ten good drops from each subject, he did not attain this aim. Many very excellent reflexes could not be measured because they were off the record. A number had to be rejected because of some disturbing factor such as: 1) an anticipatory drop just before the stimulus-drop, 2) apparatus trouble, 3) an extraneous disturbance like a cough or noise immediately before, during, or just after the stimulus, 4) evidence that the subject had moved his fingers in the cups at the sound of the stimulus, 5) a decidedly irregular curve just before the stimulus.

Fig. 6

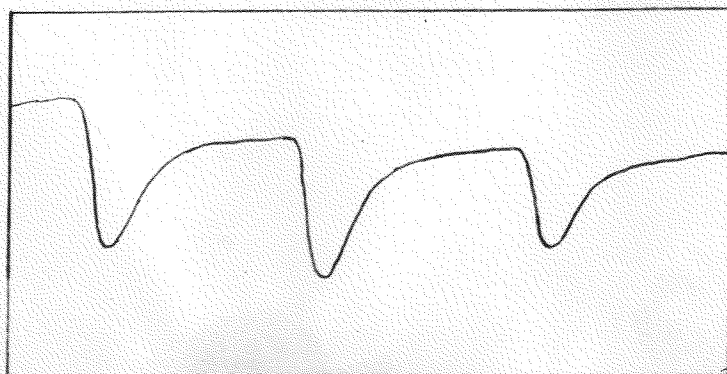
Example of drift. Stroke at top indicates stimulus. Dotted line is reference line for measurement. Time lines omitted.



Example of irregularity.



Example of incomplete recovery. No stimulus marks on this part of record.



To explain these factors more in detail, the anticipatory drop was prominent in the first few records until the experimenter found a way of manipulating the stimulus-control so as to keep the noise of the motor from serving as a warning. Even so, anticipatory drops sometimes occurred just as the experimenter had judged the galvanometer light to be steady and had pressed the stimulus-control button. The apparatus trouble consisted mainly in a prolonged stimulus caused by a futile attempt to stop the control mechanism when one of the previously mentioned anticipatory drops was detected. The only evidence of motion of the fingers consisted in an up-swing of the curve which was too sharp to be explained as a sudden spurt of further recovery; this indicated a sudden upward jerk of the fingers so as to remove them partially from the cups. The reverse, if it occurred, could not be detected. Though the evidence was not always certain, in doubt the drop in question was rejected. Inspection of the various percentages of recovery showed that those drops, before which a spontaneous reflex had occurred just before the stimulus and from which the subject had not completely recovered, were frequently widely deviant from the rest of the subject's scores --unless, of course, the subject were of the irregular type. For this reason, these drops were rejected.

Aside from disturbances, drops of less than 150 ohms were rejected because it was feared that they would distort the scores. Even generally quick reactors showed a fairly slow drop

and recovery when the drop was very small. However, when the majority of the subject's drops were small, those under 150 ohms and above 80 were counted, since they seemed quite a part of the subject's pattern.

After all these rejections, added to the limitations created by the gradual exhaustion or adaptation of the reflex and the desire to restrict the time of the experiment to about 15 minutes, the number of drops used in the final analysis of the data was less than we had hoped. There were, however, at least six drops for each subject. The complete breakdown of the number of subjects and the number of drops is presented in Table VIII. Two records were completely rejected.

TABLE VIII

NUMBER OF DROPS MEASURED PER SUBJECT

No. of Drops:	6	7	8	9	10	11	12	13	14	15	Total
No. of Subjects:	6	6	12	2	9	3	2	4	1	1	50

Reliability of the Measure

The first question we ask ourselves is the degree of error in measuring each drop. This cannot be determined accurately for the present experiment, but it is estimated at about 2.5 per cent under the best conditions. This estimate is based on those errors which were discovered in checking the measurements, and it means that, supposing that the apparatus error, the errors from the various disturbing factors that were not detec-

ted, and the errors in determining the reference point for the measurement (as explained above in reference to drift) all balance out, the error in the final figure of such-or-such per cent is probably not more than 2.5 per cent either way. Since the class interval used in many of the correlations was 5.0 per cent, this measurement error is no greater than the possible error of statistical computation. However, the estimate of error is rather tenuous.

A preliminary evaluation of the reliability of the measure in the usual statistical sense was made by comparing the range of the means with the range of each individual's single scores. The means ranged from 11.8 per cent to 85.8 per cent. The data on intraindividual range are presented in Table IX. The actual termini of the range are not given, but rather the extent of the range: i.e., the difference between the two termini.

TABLE IX

RANGES OF THE SUBJECTS' PER CENT RECOVERY SCORES

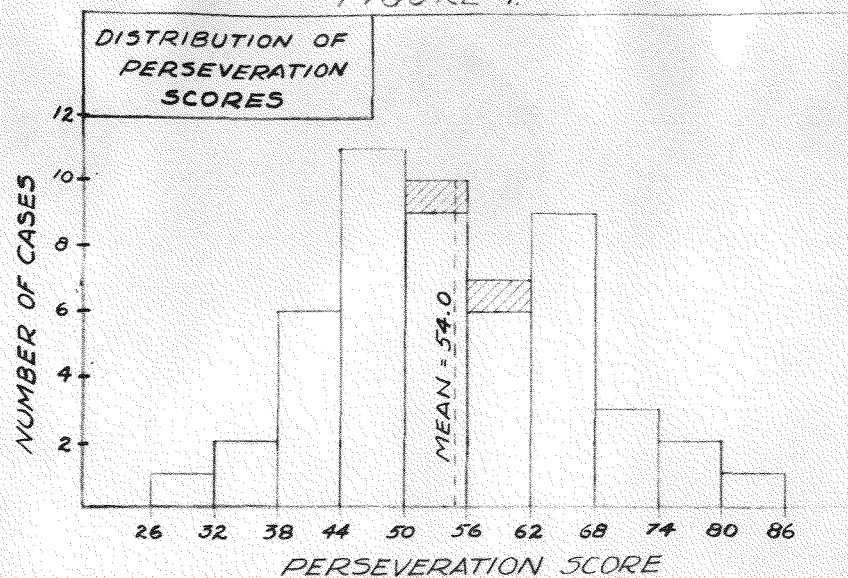
Extent of Range	No. of Subjects
6-10	1
11-15	6
16-20	8
21-25	13
31-35	6
41-45	0
<u>46-49</u>	<u>2</u>

Mean Range: 28.3

One can conclude that the range of scores for each individual, while fairly large, is not such as to exclude all possibility of reliability of measurement.

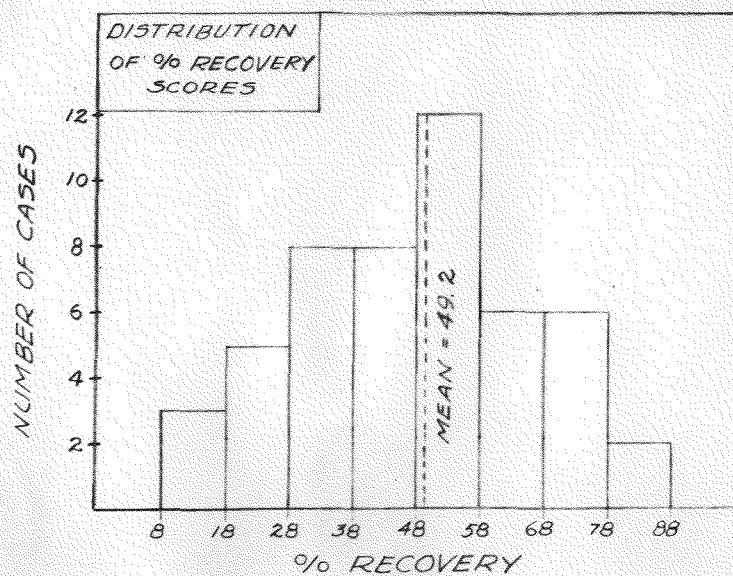
A more accurate estimate was made by the usual split-half method, which yielded a coefficient of .931, with a standard error of .143. N , here, was 50. The coefficient is very good for this type of work, and indeed compares favorably with that for a great number of paper-and-pencil tests, especially in the personality field. One difficulty in accepting this figure is, however, the fact that there were only from six to fifteen drops to "split" in the first place. Some of the averages thus correlated are therefore dependent on just three measures. On the other hand, it is precisely this fact which should increase the chances of variation between the pairs of scores. This reliability was obtained only after we had eliminated as many sources of error as possible. It seems, therefore, that we have a measure which is quite stable at the time of testing. As to the further question of its reliability from day to day, the verdict is decidedly the reverse; but this matter we shall postpone till later, when we discuss the results of the repeated testing.

FIGURE 7



CROSS-HATCHED AREA REPRESENTS THE TWO SCORES EXCLUDED FROM CORRELATION WITH PER CENT RECOVERY.

FIGURE 8



Another point of interest is the distribution of the perseveration and recovery scores. This is presented graphically in Figures 7 and 8. The perseveration scores of the two subjects excluded from the per cent recovery calculations are added, though they were not included in computing the mean indicated in the histogram. Table X gives more of the distribution statistics.

TABLE X
PERSEVERATION AND PER CENT RECOVERY SCORES

	<u>Perseveration</u>		Per Cent Recovery
	Entire Group (N=52)	Excluding Two S (N=50)	Excluding Two S (N=50)
Mean	53.9	54.0	49.4
S. D.	11.4	11.6	18.3
Range	26-81	26-81	11.8-85.8

The correlation between perseveration and per cent recovery was $-.289$, with a standard error of $.143$. The confidence level is 5 per cent, which is not sufficient. The negative sign indicates that in our data as the perseveration score increases the per cent recovery score tends to decrease, so that slow recovery tends to go with high perseveration. The relationship is not very close.

The fact that a nearly significant correlation was found when the records were analyzed qualitatively, while quantitative analysis has resulted in a correlation farther removed

from significance brings up the question of the relationship between these two methods of analysis. The correlation between the composite scores from the pooled ratings and the per cent recovery scores was .814, with a standard error of .143. Considered as equivalently a coefficient of reliability, this is only fairly satisfactory. It is not as high as the lowest coefficient obtained from the correlation of the single judges' ratings with the composite scores. These ranged from .841 to .937. The conclusion seems to be that our two methods of handling the data are not as strictly comparable as we should like, though they are fairly so. Both methods show greater internal consistency than agreement with each other.

It is necessary again to check the results of the correlation method by the comparison of means, and we now present the mean recovery per cent of the high and low perseverators. The high perseverators are here taken to be those who fall into the uppermost ten per cent on the questionnaire, while the low perseverators are those who fall in the lowermost ten per cent. These data are shown in Table XI, which includes also the data for the highest and lowest 20 per cent.

TABLE XI

RECOVERY PER CENT SCORES OF LOW AND HIGH PERSEVERATORS

1. Lowest and Highest 10 Per Cent

Low Perseverators			High Perseverators		
Subj.	Persev.	Recov.	Subj.	Persev.	Recov.
1*	26	11.8	48	71	50.5
2	33	85.8	49	72	40.3
3	36	65.7	50	74	26.6
4	38	63.9	51	79	16.5
5	39	67.0	52	81	50.9
Mean	34.4	58.8	Mean	75.4	37.0

Difference of Recovery Means: 21.8

U : 5
P : .075

2. Lowest and Highest 20 Per Cent**

Low Perseverators			High Perseverators		
Subj.	Persev.	Recov.	Subj.	Persev.	Recov.
1*	26	11.8	43	64	50.5
2	33	85.8	44	64	57.3
3	36	65.7	45	66	25.4
4	38	63.9	46	67	17.6
5	39	67.0	47	68	70.0
6	41	53.3	48	71	50.5
7	42	59.5	49	72	40.3
8	42	59.3	50	74	26.6
9	43	73.3	51	79	16.5
10	44	71.7	52	81	50.9
Mean	38.4	61.1	Mean	70.6	40.6

Difference of Recovery Means: 20.5

U : 17
t : 2.495
P : .022

* The subjects are numbered from low to high perseverators. Thus there are 52, although we do not have per cent recovery scores for two of them; these did not fall within the two groups given above.

** The data for the groups in the first part of the table have been repeated here for the sake of convenience.

The individual scores are recorded in this table because they illustrate the scatter. Subject No. 1 shows the widest deviation; he was the lowest perseverator and the lowest in recovery rate. The picture is very much the same as that presented by the correlation coefficient. The highest five perseverators are not significantly differentiated from the lowest five, since the confidence level is not even 5 per cent. When we consider the upper and lower ten subjects, the confidence level is considerably better, just short of the 2 per cent level, but the difference is still not significant. It is clear that the relationship, whether judged by the correlation or by the significance of the difference between the means of the extremes is neither close nor statistically significant.

C. OTHER FACTORS

The following section will be taken up with data relative to the various uncontrolled factors which may have influenced the results. We shall begin with a consideration of the extent of the reflex drop in resistance.

Extent of Drop

In view of the wide differences between individuals in the group in the intensity with which they reacted to the stimuli, it is quite appropriate to ask whether the recovery time was appreciably influenced by the extent of the drop from which recovery had to begin. This possible influence can most conven-

iently be estimated from the correlation between the subjects' average drop in resistance and their average per cent recovery after five seconds. The correlation was .015, which, with a standard error of .143, is of course not significant. For the purposes of correlation, only those drops were counted which were used for getting the average recovery per cent. The group average is of incidental interest, though its value is reduced by the above limitation; no more accurate computation was possible, since many drops went off the record. The group mean was 396.9 ohms; the standard deviation, 156.2 ohms; the range, 125.8 to 809.3. The lower limit of the range is exclusive of the one subject who showed only two clear drops of 44 ohms. Since the above correlation was practically zero, we can conclude that our results, at any rate, were not distorted by the size of the drop.

Age

It is generally desirable to investigate the relationship between age and the experimental variables. The correlations between the age of the subjects and perseveration, the composite qualitative ratings, and per cent recovery are presented in Table XII.

It is at once apparent from this table that in our group age was uncorrelated with either perseveration or the two measures of rate of recovery.

TABLE XII
CORRELATION OF AGE WITH THE EXPERIMENTAL VARIABLES

<u>Variable</u>	<u>r</u>	<u>S. E.</u>
Perseveration	-.058	.140
Composite Ratings	-.142	.140
Per Cent Recovery	.024	.143

Health, Fatigue, Anticipation, and Time of Day

The PGR is reported to be affected by the physical condition of the subject. In view of this report, a check was made of the per cent recovery scores of the subjects who mentioned some adverse physical condition as against those who mentioned no such condition. Under the heading of health, none admitted anything more than a cold, headache, or earache. For the most part, the colds were mild or in the course of remission. Fatigue or sleepiness was reported by an appreciable number of subjects. Apprehension because of the experimental situation may also be treated here, since it is, like health and fatigue, a subjective factor. By apprehension is meant a more or less intense fear of shock, of appearing foolish or emotional, of doing the wrong thing, and so on; and an effort was made, in questioning the subject, to make this point clear. The momentary reaction of fear when a horn is sounded would, of course, have been one of the psychic causes of the galvanic skin reflex itself and hardly a factor to be ruled out. Another condition, which, though in

itself extrinsic to the subject, is of interest because of a possible corresponding variation of physical condition, is the time of day. The work periods were determined by the time at which the subjects were available, and hence were restricted to the early morning, about 8:00 to 10:00, and the early afternoon, about 1:00 to 4:00. The morning group had just arrived at school and were engaged in their first laboratory period; the afternoon group had attended class for varying lengths of time in the morning, had presumably lunched, and were in the equivalent of their first, second, or third period in the afternoon. Since a detailed breakdown of the two groups according to the hour of the day did not seem to show any identifiable trend of the averages, the intact morning group was compared with the intact afternoon group. The effect of temperature, which presented greater difficulty, is left for separate treatment.

The effect, therefore, of adverse health conditions, fatigue, apprehension, and time of day was evaluated principally by comparison of means based on the per cent recovery scores, since these are directly quantified and easily admit of such comparison. In treating the qualitative data, it was decided to compare the number of men in each category who were subjected to the above conditions. The list of persons in each category was that referred to on page 169, in regard to the qualitative analysis of the data. The results of these two methods of comparison are presented in Tables XIII and XIV, each of which includes all of the factors.

TABLE XIII

HEALTH, FATIGUE, APPREHENSION, AND TIME OF DAY
IN RELATION TO PER CENT RECOVERY

Condition	Per Cent Recovery					
	N	Mean	S.D.	Diff.	t	P
Cold, headache, etc.	19	49.1	18.8			
Good health	31	46.0	17.4	3.1	.581	.549
Fatigued	20	47.5	23.8			
Not fatigued	30	46.9	12.2	.6	.183	.842
Apprehensive	28	44.4	18.8			
Not apprehensive	22	50.8	15.9	6.4	1.252	.194
Morning	20	43.4	18.2			
Afternoon	30	49.8	17.2	6.4	1.235	.230

TABLE XIV

HEALTH, FATIGUE, APPREHENSION, AND TIME OF DAY
IN RELATION TO THE NUMBERS IN EACH OF THE QUALITATIVE CATEGORIES

1. In Terms of Numbers

Category	Cold, etc.		Fatigue		Apprehension		Time		Category Totals
	Yes	No	Yes	No	Yes	No	A.M.	P.M.	
I	4	4	3	5	5	3	3	5	8
II	5	9	6	8	6	8	6	8	14
III	9	13	9	13	14	8	9	13	22
IV	1	7	3	5	3	5	3	5	8
Totals	19	33	21	31	28	24	21	31	52

2. In Terms of Per Cent of the Subtotals*

Category	Actual Per Cents				
	Expected Per Cent	Cold, etc.	Fatigue	Apprehension	P.M.
I	15.4	21.1	14.3	17.9	16.1
II	26.9	26.3	28.6	21.4	25.8
III	42.3	47.4	40.9	50.0	41.9
IV	15.4	5.3	14.3	10.7	16.1

* The expected per cent is the proportion of the entire group classified in each category. Succeeding columns represent the percentage of the subtotals falling in each category.

A cursory glance at the table of means for the per cent recovery will reveal that none of these factors affected the results significantly, since the differences could easily be attributed to chance. Similarly, a comparison of the number and percentages of the persons in each of the qualitative categories of recovery reveals that the various conditions seem to have been distributed through the four categories in about the same proportion. We use as a criterion the per cent of the number of persons placed in each category on the basis of the pooled ratings of the judges, since, if the various factors of health, etc. have not affected the persons in one category rather than another, we should expect these same percentages to be duplicated throughout the table. These percentages, in other words, are the expected percentages for their respective categories if the factors in question have not influenced the rate of recovery so as to slow it down or speed it up. The percentages under the headings Cold, Fatigue, etc., are percentages of the subtotals; i.e., of all those reporting colds, 21.1 per cent were in the recovery category No. I; 26.3 per cent, in No. II, etc. Considering the small numbers involved in each subtotal, the correspondence with the expected percentages is surprisingly good, except for those in the group reporting colds. This discrepancy is not great, again considering the small numbers involved, and does not seem to have had any systematic effect on the category ratings (No. II is not affected, although its neighbors

III and IV are). It is probably quite in keeping with the evidence from the means for per cent recovery, in which case there is a slight difference (3.1) in favor of faster recovery on the part of those who have colds, though the difference is not significant. With these considerations and in view of at least one report in the literature (106: 116 f., 168) indicating that the situation is, if anything, the reverse, we may be reasonably confident that the discrepancy is due to chance.

Temperature

The effect of temperature requires more detailed analysis. According to Landis (113: 713), Maragaria has demonstrated that the conductivity of the skin, which is related in a non-linear fashion to its temperature, increases with heat and decreases with cold. These variations would directly affect the general resistance level and probably the extent of the reflex drop, leaving, perhaps, the rate of recovery unaffected. Unfortunately our data relative to the temperature effect are somewhat distorted because of the fact that the experimenter neglected to note the temperature to which the first four subjects were submitted. These four turned out to be among the fastest recoverers by both criteria; all fell into the category of fastest recovery, while their mean recovery per cent was 73.0, with a range of 67.0 to 83.2. With this limitation in mind, we present the following data. Table XV contains the means and range at each temperature, with the standard deviation when it was worth computing.

TABLE XV

1. EFFECT OF TEMPERATURE ON PER CENT RECOVERY

Temperature	N	Mean	Range	S. D.
66	1	11.8	-	-
67	1	50.5	-	-
68	4	45.4	32.0-59.5	11.6
69	9	46.5	25.4-85.8	17.0
70	12	36.6	16.5-73.3	18.5
71	8	45.2	20.2-65.7	12.4
72	2	61.6	59.3-63.9	-
73	4	47.4	37.0-58.0	7.9
74	5	58.6	33.9-72.6	14.1
No record	4	73.0	67.0-83.2	5.8

2. SAME DATA GROUPED *

Temperature	N	Mean	Range	S. D.
67-68 **	5	46.5	32.0-59.5	10.2
69-70	21	40.8	16.5-85.8	18.5
71-72	10	48.4	20.2-65.7	13.2
73-74 **	9	53.6	33.9-72.6	13.1

* The one subject at 66 degrees was omitted for convenience of classification. The "no record" group are also omitted.

** The difference between the groups indicated by the double asterisk is 7.1, and t is .970, which is not significant.

A further consideration is the various ranges and standard deviations at different temperatures. If there is any correlation at all, the scores in any one column or row should have a smaller scatter than the scatter of all the scores on the variable in question (137:114). We find, however, that the range of scores at temperatures of 69 and 70 degrees is almost as great as that of the entire group (60.4 and 56.6 points as against 74.0), while the standard deviations are greater (17.0 and 18.5 as against 16.9).

The results of the analysis of temperature in relation to the composite qualitative ratings tend to confirm the position that the recovery scores are not appreciably affected by the temperature. The correlation in this case is about the same (.264), which is also not significant (standard error: .146). When the persons in each qualitative category are lined up, the temperatures under which they worked tabulated, and the mean temperatures for those in each category computed, we get the following results (Table XVI):

TABLE XVI

MEAN TEMPERATURE FOR VARIOUS RECOVERY CATEGORIES

Category	No. of Cases	Mean Temperature
I	4*	70.5
II	14	71.2
III	22	69.8
IV	8	70.3

* The four subjects on whom we have no record of temperature were in Category I.

Accordingly, the temperature was on the average about the same for the persons in the four categories. If temperature were systematically increasing or decreasing the speed of recovery, the distribution ought to be biased one way or the other so that the average temperature would rise as we go from I to IV, or vice versa.

A further indication that temperature differences do not account for the differences in recovery rate is afforded by our check of day-to-day variability. When the same three subjects were tested five times at varying intervals and at temperatures running from 66 to 79 degrees, no correspondence was evidenced between the temperature differences and the fluctuations of mean recovery per cent. This we can see most conveniently by tabling each subject's scores according to the order of ascending temperature (Table XVII).

TABLE XVII

MEANS OF THE SAME SUBJECTS AT DIFFERENT TEMPERATURES

Subject 1		Subject 2		Subject 3	
Temp.	Recovery Per Cent	Temp.	Recovery Per Cent	Temp.	Recovery Per Cent
66	41.9	66	68.3	67	19.1
67	45.7	67	57.1	72	46.8
68	55.6	76	64.2	77	7.1
76	32.1	78	70.7	79	57.5
77	31.7	78	83.8	79	58.6

We may conclude, therefore, that the temperature of the room probably had no more than a chance relation to the recovery scores.

Level of Resistance

Another factor in need of investigation is the subject's level of resistance. Although our measure, as previously explained, is only a rough approximation to the true resistance level, we have computed its correlation with the two estimates of recovery and with perseveration. The resistance level was read to the nearest 250 ohms for each drop used in calculating the recovery per cent. The error was thus 12.5 per cent in 1,000 ohms or 1.25 per cent in 10,000 ohms. Since only one subject averaged as low as 10,000 ohms, the error never exceeded 1.25 per cent.

The pertinent correlations are given in Table XVIII.

TABLE XVIII

CORRELATIONS RELATIVE TO RESISTANCE LEVEL

	<u>r</u>	S. E.
Resistance Level & Per Cent Recovery	-.298	.143
Resistance Level & Ratings*	-.230	.140
Resistance Level & Perseveration	-.025	.143

* N here is 52, since the two records which could not reliably be measured could, however, be rated.

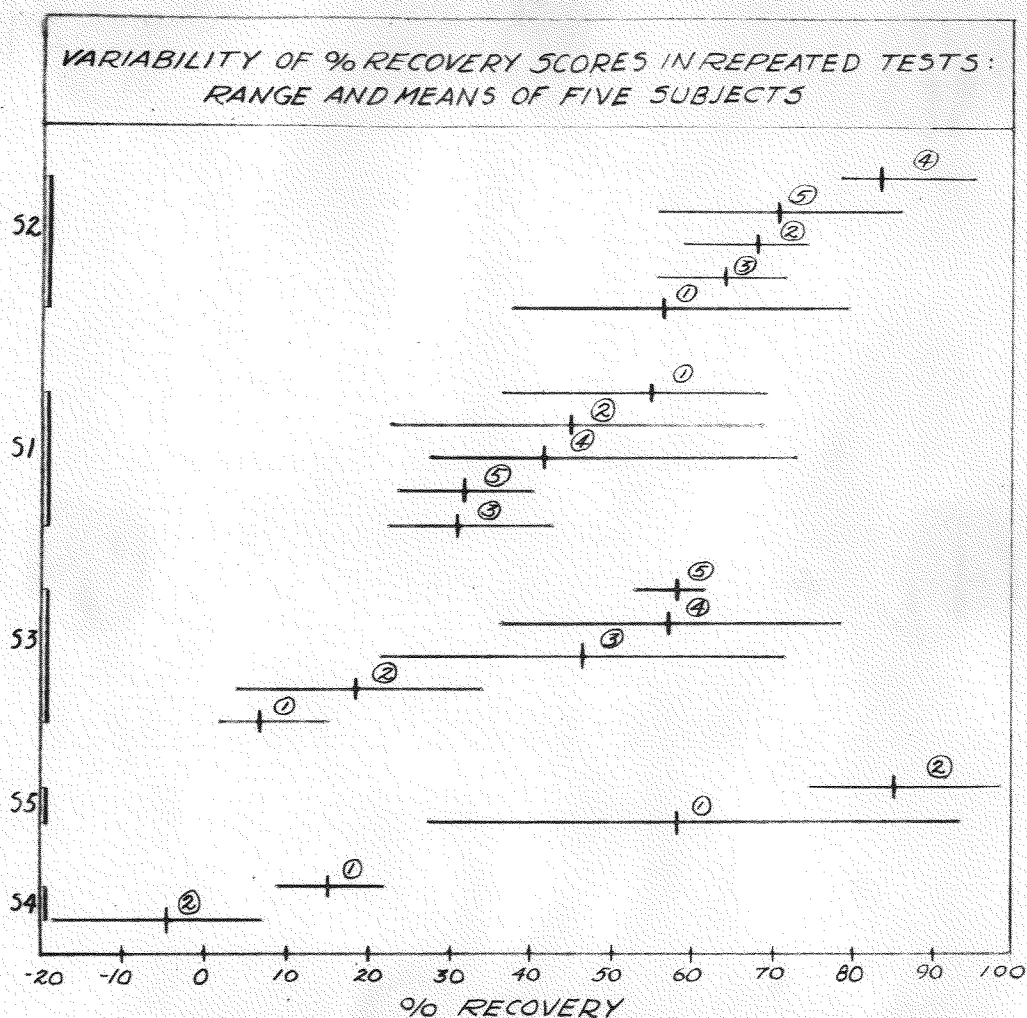
From these correlations we may conclude that the relation between the average resistance level and the average recovery per cent is

slight and easily attributable to chance. The almost zero correlation of perseveration with resistance level is surprising. If the latter depends, as Darrow (102) and Haggard and Garner (110) claim, on the general level of excitation, we should expect the higher perseverators to maintain a higher level of excitation and hence a lower level of resistance. The failure of this expectation may be due to the inaccuracy of our measure of resistance level, but it is suggestive of further research.

D. CONSTANCY OF RECOVERY RATE FROM DAY TO DAY

The data available for a study of the constancy of the two measures of recovery consists of the measures and ratings for three subjects who were run through the experiments five times over periods of 9 to 35 days, and of the measurements for two subjects who participated in the final and in one of the preliminary experiments. The results of the quantitative analysis of these data are presented in Fig. 10 and Table XIX; the results of the qualitative analysis, in Table XX.

FIGURE 10



THE RANGE IS INDICATED BY THE LENGTH OF THE LINE;
THE MEAN BY THE VERTICAL STROKE. THE ENCIRCLED
NUMBER ABOVE THE LINE INDICATES THE TESTING
SESSION, I.E. FIRST, SECOND, THIRD, ETC.

TABLE XIX

DAY-TO-DAY VARIABILITY OF PER CENT RECOVERY SCORES

			Recovery Per Cent		
	Day	Drops	Mean	Range	Range, All Scores
<u>S 1:</u>					
	1*	6	55.6	37.1-68.6	
	2	7	45.7	23.4-68.8	
	4	5	31.7	23.3-42.4	23.3-73.3
	6	9	41.9	28.1-73.3	
	<u>10</u>	<u>7</u>	<u>32.1</u>	<u>24.0-40.1</u>	
Means**			41.6	31.7-55.6	
<u>S 2:</u>					
	1	7	57.1	37.6-79.4	
	3	6	68.3	59.3-75.0	
	7	8	64.2	56.1-71.5	37.6-95.2
	31	8	83.8	78.9-95.2	
	<u>35</u>	<u>6</u>	<u>70.7</u>	<u>56.2-86.0</u>	
Means			69.1	57.1-83.8	
<u>S 3:</u>					
	1	4	7.1	2.2-14.9	
	2	2	19.1	4.1-34.1	
	4	2	46.8	21.8-71.8	2.2-78.6
	15	2	57.5	36.4-78.6	
	<u>29</u>	<u>3</u>	<u>58.6</u>	<u>53.4-61.4</u>	
Means			34.7	7.1-58.6	
<u>S 4:</u>					
	12/47	8	58.9	27.7-93.9	
	3/48	<u>10</u>	<u>85.8</u>	<u>75.2-98.5</u>	27.7-98.5
Means			73.8	58.9-85.8	
<u>S 5:***</u>					
	10/47	5	15.8	9.1-22.2	
	3/48	<u>6</u>	<u>-4.0</u>	<u>-18.2 to +7.5</u>	-18.2 to + 22.2
Means			4.1	-4.0 to +15.8	

* The days are numbered so as to indicate the time intervals. The subjects did not begin the series on the same day.

** These means are weighted according to the number of drops.

*** This is one of the two subjects excluded from the per cent recovery computations, the reason being that the minus recovery did not seem to fit the actual fact as represented by the curve pattern. Cf. p. 172.

TABLE XX

DAY-TO-DAY VARIABILITY QUALITATIVELY JUDGED

Subject	Day	Mean Normal Scale Value	Mean Category	Agreement of Judges
<u>S 1:</u>	1*	-.44	II	100%
	2	-.44	II	100%
	4	.75	III	75%
	6	1.26	IV	75%
	10	<u>.75</u>	<u>III</u>	<u>75%</u>
	Mean	.38	III	85%
<u>S 2:</u>	1	-1.56	I	100%
	3	-1.56	I	100%
	7	-1.28	I	75%
	31	-1.56	I	100%
	35	<u>-1.56</u>	<u>I</u>	<u>100%</u>
	Mean	-1.50	I	95%
<u>S 3:</u>	1	1.52	IV	100%
	2	.75	III	75%
	4	.75	III	75%
	15	.05	III	75%
	29	<u>-.44</u>	<u>II</u>	<u>100%</u>
	Mean	.53	III	85%

* The days are numbered so as to indicate the time intervals. The subjects did not begin the series on the same day.

' One judge's rating differed from the rest by two categories.

It can be seen from these two tables that the variability from day to day was, by and large, considerable. One subject, S2, proved almost completely consistent if we consider the judgment of the records by inspection; he was not consistent, if we go by per cent recovery, since the means ranged thru 26.7 points: i.e., from close to the mean (45.0%) of the experimental group of 50 subjects to practically the upper extreme, which was 85.8%. In the case of Subject 1, two records clearly belonged in the second fastest recovery category, while the others belonged to the third or the last. His per cent recovery means ranged through 23.9 points, the limits lying close to one S.D. below and one S.D. above the mean of the larger group. Subject 3 showed extreme variability. The four judges agreed perfectly on two of his records, placing one in Category II and the other in IV. Another record of his probably belongs in the second category, though the average of the judges' ratings does not correspond; this is the case, indicated by the note in Table XIX, in which one judge placed the record in the fourth category, thus throwing the mean from II to III. The remaining two records were assigned to III and IV by the different judges. That this is the subject's rather than the judges' variability, seems to be shown by the equal variability of his per cent recovery scores. His mean scores range through 51.5 points, from rather close to the scale's zero point to somewhat beyond the mean of the larger group.

It will, perhaps, throw light on the broader topic of restricted reflexes if we discuss this subject's records more at length. It was difficult to elicit a reflex from him, and no stimulus was likely to be effective after it had once been used in any one session; hence he shows only a handful of drops all told by which to form a judgment or from which to derive a measure. He does preserve consistency from day to day in that he does not react more than occasionally, and then generally with a rather small drop. In regard to recovery, he is anything but consistent from day to day. Shall we assume that, had he given more drops in any one session, they would have followed the pattern of the two, three, or four that we have? Or shall we assume that his variability is equivalent merely to the variability we find in one record made in one session with some more reactive subject? Actually, when we allow for disturbances in the course of the experiment, as previously discussed in regard to the larger group, we do not find quite such a large range for a single subject in a single session; and even when we do not rule out these disturbances, we do not get such decided differences in pattern as that between this subject's first record and his last. It seems likely that there is a genuine variability of speed of recovery from day to day in his case.

We might be inclined to look for an explanation of his great variability in the fact that all sorts of stimuli had to be used with him in order to get any reflexes at all. However,

the stimuli that were effective were very much the same as those used with the larger group. Only two of the effective stimuli were verbal; the rest were those used with the larger group, except for the discharging of a cap-gun. This latter, when it was effective, elicited the same pattern of response from him as did the regular stimuli within the same session. As a matter of fact, the change in pattern, though not the change in recovery per cent, first occurred in the fourth session, before which the subject remarked: "This time I won't try to control the reaction," or words to that effect. This remark, though not in harmony with the fact that he still did not give many drops, may indicate that a change in attitude is responsible for the decided change in pattern. The change in recovery per cent, however, began at least with the third record. Perhaps habituation to the experiment had something to do with this in his case, although with subjects 1 and 2 there did not seem to be any such effect on the per cent recovery score itself -- there was, of course, adaptation to some of the stimuli.

It will probably not be taken amiss if we digress for a moment and discuss the possible implications of this subject's remark about controlling the reflex. He apparently made an effort to do so in the earlier sessions. In those sessions, he gave a pattern that has been mentioned in the literature as frequent in catatonic patients (111, 114). The thought that occurs to one is that this characteristic of catatonics may be due

to a more or less conscious attempt at control of emotional manifestations on a par with the rigidity of posture they frequently maintain. This would be an interesting subject of further investigation.*

The means of S4 and S5, the two subjects who participated in one of the preliminary experiments and in the main experiment, also indicate decided variability. Thus, S4's mean for the one session was 58.9, which would place him about one S.D. above the mean of the main group, whereas his mean in the main session itself was 85.8, the highest score in this group. S5 did not vary so much; his first mean was 15.8, but in the main experiment his mean was -4.1. Perhaps it would be more accurate in the case of this subject to say that his variability had the effect of changing the record from an ordinary one to one that was quite uninterpretable. This was the subject who made a deliberate effort to relax by fixating a spot on the wall. Thus we have another indication that attitude may affect the response. The two records of these subjects could not be compared for shape of recovery curve, since the circuit used in the preliminary experiments did not guarantee constant current and voltage at the galvanometer.

* Some time after writing the above lines, the author came upon the article by Hoch, Kubis, and Rouke (114), in which there is some evidence to confirm this suggestion. Distracted normals gave a pattern of response similar to that of catatonics.

In addition to the above evidence of considerable day-to-day variability of the recovery per cent and of the pooled judgments of predominant curve type, we have the scatter of single scores and the scatter of the ratings given the records by the single judges. These data are presented in the Figures 11 and 12.

In explanation of the graphs, it is to be noted that the recovery per cent scores were grouped according to class intervals of ten per cent, and the datum point was plotted above the mid-point of the interval. In plotting the data from the judges' ratings, each category was simply assigned a convenient and equal interval along the base line. The ordinate represents the number of times any record of the given subject was placed in a given category; the sum of the frequencies thus indicated on the ordinates for any one subject would be 20, since his five records were classified by four judges, making a total of 20 times that judgment was passed on him. The categories are numbered from right to left along the abscissa for convenience in comparing the two graphs, since Category I means the fastest type of recovery and hence corresponds to a high recovery per cent, while Category II represents a bit slower type of recovery and therefore a slightly lower recovery per cent -- and so on for the rest.

Fig. 11
Distribution of Single Ratings Given Records
of Three Subjects in Repeated Tests
(Legend: Cf. Fig. 12)

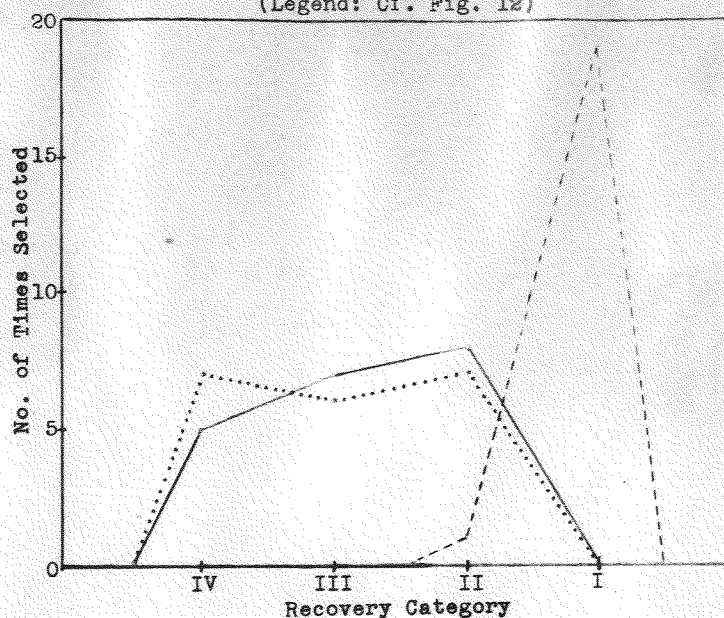
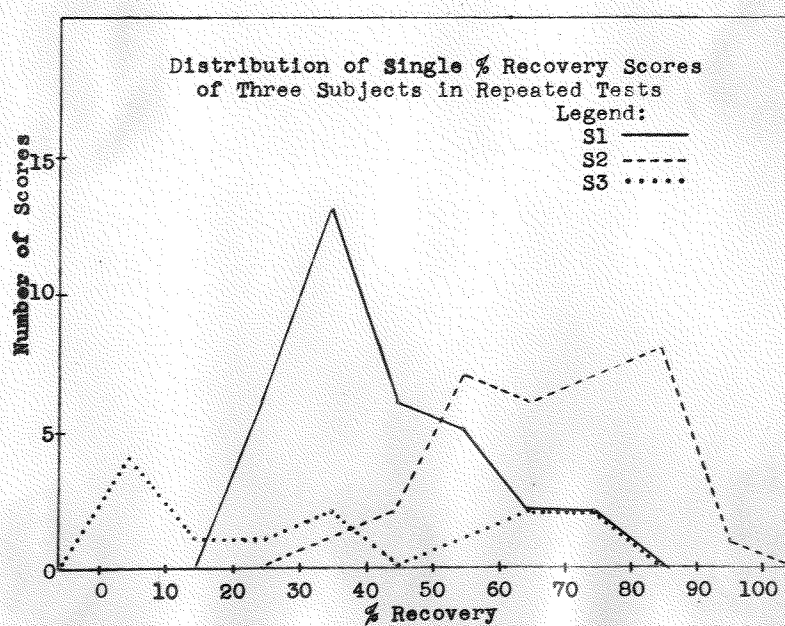


Fig. 12



These figures confirm the tabled data on the scatter of means, especially in regard to Subject 3. In his case there is practically no indication of a tendency for the recovery per cent scores to group themselves at any point on the scale; as to placement in categories, the only indication of a central tendency is the fact that he was never placed in Category I. The other two subjects show a central tendency in their per cent recovery scores, but show also a large scatter and an appreciable overlap. In regard to the categories, S1 shows something of a central tendency, but it is not very definite. S2 shows a clear-cut central tendency.

From these data, we must, it seems, conclude that the recovery from the psychogalvanic reflex is not sufficiently constant for various subjects from day to day to allow us to use it as a measure of a stable trait, or to permit correlating it with any other trait. With some subjects it may be constant enough to retain the same general pattern, as in the case of Subject 2; but with other subjects it will be extremely variable by either the qualitative or the quantitative method of evaluation. It does not seem probable that any other quantitative method applied to our records would have yielded a more constant measure. The mathematical analysis of the whole curve, as suggested by Darrow (104), would perhaps show a little less variability than our qualitative analysis, since it would eliminate subjective factors; but it is clear that

there is so much obvious variation in the shape of the curve that the improvement would not change matters much.

The results of our investigation of the constancy of the recovery measures are most comparable with those of Cattell (98), who reports considerable variation in the shape of the curves for a given subject in often-repeated testing. We cannot compare our results directly with those of Lauer (118) or of Wechsler, Crabbs, and Freeman (126) because we were unable to compute coefficients of test-retest reliability and because we used different measures. Lauer's r 's of .619 and .522 seem to have been derived from two measures which should be related arithmetically, so that identical coefficients should have been found. (The measures were the extent of deflection and the change in ohms.) This discrepancy makes one a little cautious in accepting his results. By a statistical procedure which he does not clearly describe, he gets corrected coefficients of .78 and .72, with .74 for a third measure (the per cent change). We should be willing to grant this contention that these are better than those derived from paper-and-pencil tests of emotionality, but they still leave much to be desired.* Of several correlations given by Wechsler et al., the highest is .727 for

* If one were to predict a subject's second score from the first, the improvement over a best guess would be about 35 per cent for these coefficients.

the median amplitude of response. This also does not appear quite satisfactory. They give one very low coefficient, .194, which is for the ratio of the number of responses to the total time. The difference between these two coefficients may serve to give point to our hesitancy in regarding the data on amplitude of response as comparable to ours on the recovery time. Similarly the high reliability coefficient of Welch's and Kubis' conditionability measure, .88, does not guarantee the reliability of a totally different measure (127).

All in all, it seems that our evidence points to such great individual variability in recovery from day to day that no great faith can be placed in any measure or estimate of recovery time. Our data are, it is true, based on only five cases. However, they are so consistent that it would take at least 15 subjects, all showing high constancy, to reverse these findings definitely. Only one of our subjects showed high consistency, and that in only the less accurate of the two methods of analysis. If, then, we were to add 15 subjects who showed little variability, the count would then be 16 to 4 or 15 to 5 in favor of day-to-day constancy. Would this be great enough? The numbers of such a sample would, incidentally, be similar to those employed by Lauer and Wechsler et al. (22 & 19 subjects respectively.)

CHAPTER VIII

CONCLUSIONS

In evaluating our findings, we may turn our attention first to the test of introspected perseveration. It appears that the questionnaire is reasonably discriminative. In the final scoring, after nondiscriminative items had been eliminated, and with the final experimental group, there was an actual range of 26 to 81 points. Although the latter is 49 points below the maximum score possible, this is not strange in view of the fact that the group was comparatively select. For the correlational item analysis, the range among the 200 protocols was 33 to 112. As is usually the case, there is considerable overlap among individuals in the middle of the range, as may be gathered from Table XXI, which is presented in the appendix.

The reliability of the questionnaire for the experimental group is rather good, but not entirely satisfactory. The raw coefficient, .689, is far too low in itself. This is very likely due in part to the fact that the number of items available for each of the two correlated halves is only 13. This is a serious drawback with a test of this kind, in which the subject's

error of memory or of interpretation is likely to be relatively great for any single item. Another factor that may account for this low coefficient is the fact that some of the items are quite less reliable than others. The number of poorly reliable items, as judged by the low correlations with the total score minus the item, was not equal for the two halves. Because of these considerations, we are inclined to accept the corrected coefficient of .816 as probably the better indicator of the test's reliability. Even this is not as high as we should like. We have no data on test-retest reliability. Since this is usually lower than the split-halves reliability, we may infer that our questionnaire would suffer the same fate.

That the various items measure the same thing is indicated by the fact that the discrimination indices of the retained items are positive and that the correlations of these items with the total score are also positive. Both of these points require elucidation. The discrimination index is based really on the degree to which the single item agrees with the pool of the items, since we determine the index by first selecting the two extremes of the total-score distribution and then computing the average score of these groups on the single items. If an item is not related to the other items, the scores on it will be distributed independently and hence its upper and lower extremes will not correspond with those of the pool. Hence, the subjects who lie at the extremes for the pool of items will be scattered randomly

throughout the distribution for the single unrelated item. When the averages are then computed as we have done, the item averages for the upper group and lower group by total score are likely to be the same or almost the same. Hence a low discrimination index, which is the difference between the two averages, indicates that the item in question is unrelated to the other items and is measuring something else.

Similarly, in regard to the correlations, any coefficient which is positive and significant indicates that there is a greater than chance relationship. There is a systematic factor which is causing the item score to correspond to the total score. In view of the nature of the bulk of the questions, this something may reasonably be designated perseveration. It is true, of course, that we have retained at least two items that might have only a chance relation to the rest. The fact that the test was more reliable with these items retained is hard to interpret. Perhaps the explanation is that merely by the addition of one item for each of the halves to be correlated the total score was made slightly less dependent on variations of score from one item to the next.

The validity of the items we can only infer from their nature and the fact that they have been shown to go together. Their nature indicates that they tap by and large the class of phenomena called perseverative. That these phenomena are real cannot be denied. The fact that we have other than zero scores on

these items is sufficient guarantee of this. Hence, therefore, when the correlations indicate some kind of unity among these phenomena, it is clear that we have put our finger on some one reality. The name of this reality is immaterial. Since there is practically universal agreement in calling the bulk of these manifestations and the underlying tendency by the name of perseveration, we feel justified in retaining the term and in regarding the questionnaire as a valid test of perseveration.

In regard to the reliability of the two measures of speed of recovery from PGR and the relation of this phenomenon to perseveration, we shall first try to summarize our results as they are, prescinding for the moment from the question of day-to-day variability.

As to the possibility of reliable measurement of speed of recovery, we find that, in a series in which each subject is given only one sitting, there are great individual differences between the extremes of slow and fast recovery. This can be seen by mere inspection of the records. Four categories can be assigned: I. Very fast, II. Moderately fast, III. Slow, IV. Slow and incomplete. Records belonging in the first and fourth categories are easily classified; those belonging to the middle two, not so readily. There is good agreement among judges. Great individual differences in recovery are also found if a measure of recovery speed is taken in terms of per cent recovery five seconds after the cessation of the drop in resistance. The variability

of any subject's mean recovery score is fairly great, but not excessive, if care is taken to exclude instances in which some disturbance has occurred or the drop has been very small. The reliability of this measure is good, since the split-halves coefficient is .931 (uncorrected).

When recovery speed is estimated qualitatively, i.e., by classification of the records according to the four categories mentioned above, the evidence of a negative correlation between recovery speed and introspected perseveration is suggestive, but not conclusive. The correlation obtained in this study was $-.346$, which had a standard error of $.140$ and was significant at the 2 per cent level and almost the 1 per cent level. If this correlation were significant --and supposing that recovery rate were to remain constant from day to day --this would indicate that there is a real but slight tendency for the high perseverator to take more time than the low perseverator to recover from the effect of a reflex drop in resistance. When the mean perseveration scores of the subjects falling into the fastest and slowest of the four recovery groups were computed, there was a difference between the two means which was significant at the 1 per cent level. This would indicate a significant relationship between the two variables. The evidence is somewhat inconsistent.

The correlation between perseveration and per cent recovery was $-.289$, of which the standard error was $.143$. Here the confidence level was 5 per cent, and hence the correlation is far

from significant. When the mean per cent recovery scores of the highest and the lowest perseverators are compared, the difference is not significant at any level appreciably better than 5 per cent. Therefore, these two lines of evidence are in agreement in indicating no significant relationship.

While the discrepancy between the two methods of analysis of the galvanometer records, quantitative and qualitative, is of some interest, there is little point in discussing it, in view of our finding in regard to day-to-day variability. The quantitative measurement is probably the more accurate and reliable, and hence to be given the preference. In that case, the verdict would be against a better than chance relationship between introspected perseveration and rate of recovery from PGR.

From the results of the check on the day-to-day constancy of the recovery rate, we conclude that the rate of recovery is not a constant characteristic, but varies over even a comparatively brief period of time. The force of the evidence of variability is best appreciated by considering the change in position relatively to the larger experimental group as we take now the one extreme of a subject's means, now the other. All of the five subjects varied at least one standard deviation unit. One varied from about the mean to practically the upper extreme; another from one S. D. below to one S. D. above the mean; a third, from close to zero to somewhat higher than the mean; a fourth, from one S. D. above the mean to the upper extremes; the last, from a

minus score to a little above the lower extreme.

The constancy which appeared to hold for the single sitting may well reflect the subject's temporary condition and as such may be valuable in itself, but it does not reflect a stable trait. Hence any attempt to relate the speed of recovery from PGR to personality or other traits which are relatively stable seems doomed to failure. That introspected perseveration is a constant trait we do not maintain without proof, which to the present writer's knowledge has not yet been given. However, the perseveration measured by the questionnaire possesses relative constancy, since the answers represent the remembered perseverative manifestations in the ill-defined but relatively long period covered by one's memory at the time. This may not be longer than several months, but it is certainly relatively long in comparison with the week or month during which we ran the repeated PGR measurements.

The fact that the recovery rate gave evidence of being rather constant during one sitting suggests the possibility that it is a good measure of some temporary condition. What this condition may be we can only guess, but it would be worth investigation and might prove a fruitful field of research. Another, point worth investigating is the effect of attitude on PGR, as regards frequency of occurrence, extent of resistance drop, and rate of recovery. Our data on the relation of temperature to the measures employed, while they indicate no more than a chance

relation, suggest the advisability of controlling this factor in future research rather than adopting the expedient of recording it and hoping to partial out its effect.

Besides temperature, the possible influence of other factors has been considered, with the conclusion that the data indicated only a chance and slight relationship. These factors were: the extent of the drop, age, health, fatigue, anticipation, and the time of day. Our data on health are the least valuable for conclusions beyond the experimental group, since there was question only of minor indispositions like a cold or a headache. Besides this, we had only the subject's report of how he felt. Similarly, we should not think of concluding that fatigue is generally unrelated to recovery rate, since we had no accurate means of estimating that factor.

In regard to the hypothesis we set out to test, it appears that the results of this experiment indicate that introspected perseveration is not dependent on a general tendency of the autonomic nervous system to persist in activity once it has been aroused, and that hence there can be no dependence on such a characteristic of the entire nervous system. We do not wish to say anything about the somatic nervous system, but we do wish to submit that the autonemics must be excluded. This conclusion is, of course, tentative and subject to the limitations of this experiment. Prominent among these limitations is the fact that only one of the manifestations of autonomic activity has been investigated. However, it is a reasonably good index

of the activity of either the entire autonomic system, or of the sympathetics only, or of the parasympathetics only. Hence, from this standpoint, the conclusion seems to hold water: there is no tendency to persist in activity that prevails universally in the autonomies and is related to perseveration.

Aside from any question of relationship to perseveration, there is no constant or stable tendency in this branch of the nervous system to persist in activity to a definite, fixed degree. The degree of persistence of activity is relatively constant, at least for most of the individuals in a group like ours, under certain conditions and at a certain time, but does not remain constant over a longer period of time. Whether this is ultimately due to a physiological variability, apart from any environmental change which may have physiological repercussions, or whether it is merely the result of such environmental changes we cannot say.

CHAPTER IX

SUMMARY

1. An experiment was devised to test the hypothesis that perseveration as manifested introspectively is dependent on a universal tendency of the nervous system to persist in activity once it is aroused. The autonomies were selected in order to provide a negative test of this hypothesis, since if the hypothesis is valid it must apply also to the autonomies.

2. A questionnaire of 40 items was constructed and standardized with a group of 463 college students, using the Likert technique of internal consistency. The 26 items retained were also correlated singly with the total score minus the item in question, using a sample of 200 protocols. The questionnaire proved discriminative, fairly reliable, and probably valid in that the single items measure a common tendency, to which the name of perseveration has generally been given.

3. The questionnaire was administered to 52 undergraduate students of psychology at Loyola University in the spring of 1948. These students were also run through a series of auditory stimuli while connected to the psychogalvanometer. Their rate of recovery from PGR was estimated qualitatively by six judges who placed the photographic records in four categor-

ies of recovery according to a set of norms. An average rating for each subject was computed by means of normal scale values. The recovery rate was estimated quantitatively by means of the per cent of recovery five seconds after cessation of the reflex drop in resistance.

4. The correlation between perseveration and the rate of recovery as estimated qualitatively was $-.346$; its standard error $.140$; its significance level, 2 per cent. That between perseveration and per cent recovery was $-.289$; its standard error, $.143$; its confidence level, 5 per cent. Neither correlation is significant.

The upper extreme for slowness of recovery, as estimated qualitatively, differed in mean perseveration score from the lower extreme by 16.9 points, a difference which is significant at the 1 per cent level. However, the upper and lower extremes, arranged according to perseveration score, did not differ significantly in mean per cent recovery score.

5. Temperature, the extent of the resistance drop, age, health, fatigue, anticipation, and the time of day proved to have no more than a chance relationship with the recovery rate. The correlation of temperature, however, with the two measures was comparable to those given above for the principal variables, being $.264$ (standard error: $.146$) for the qualitative estimates and $.292$ (standard error: $.149$) for the per cent recovery scores. This emphasizes the need of control of temp-

erature in studies of recovery from PGR.

6. When three subjects were put through five experimental sessions each, over periods of 9, 29, and 35 days respectively, their per cent recovery scores varied so widely that there was no evidence of a constant, stable, characteristic rate of recovery. Qualitatively, the successive records showed considerable difference, except for one of the subjects. Two other subjects participated in the final and preliminary PGR experiments. Their per cent recovery scores for the two series also showed considerable variation. Hence there does not appear to be sufficient day-to-day constancy to allow one to regard the recovery rate as a stable characteristic of the individual.

7. The principal conclusions were:

a. The autonomies do not maintain a constant degree of persistence of activity from day-to-day, in as far, at least, as this can be determined by the rate of recovery after the psychogalvanic reflex.

b. Hence any fixed relationship with even a relatively constant trait like perseveration as measured by our questionnaire is impossible.

c. Perseveration is not related to a constant, fixed tendency of the autonomies to persist in activity once aroused, because no such tendency exists. This latter finding settles the doubt left by the inconclusive and conflicting evidence regarding such a relationship afforded by the correla-

tions and comparisons of means.

d. Since the autonomies, or at least either the sympathetics or the parasympathetics, must be excluded from any supposed universal relationship of the nervous system to per-severation, the hypothesis of such a relationship to the entire nervous system is not verified.

BIBLIOGRAPHY

A. PERSEVERATION

1. Ach, N. Zur Lehre von der Perseveration. Z. Psychol., 1926, 12, 193-267.
2. Aristotle. De memoria, 2, 28-30. I. Bekker (ed.). Aristotelis Opera. Vol. III, pp. 287-288. Oxford: Oxford University Press, 1837.
3. Bernstein, E. Quickness and intelligence. An enquiry concerning the existence of a general speed factor. Brit. J. Psychol. Monogr. Suppl., 1924, 3, No. 7.
4. Biesheuvel, S. The nature of temperament. Trans. roy. Soc. S. Africa, 1936, 23, 311-360.
5. _____. The measurement of the threshold for flicker and its value as a perseveration test. Brit. J. Psychol., 1938, 29, 27-37.
6. Burri, Clara, The present status of the problem of individual differences in alternating activities. Psychol. Bull., 1935, 32, 113-139.
7. _____. Individual differences in ability to alternate activities. J. gen. Psychol., 1934, 10, 344-363.
8. Cameron, D. E. & Caunt, T. G. B. Studies in perseveration. J. ment. Sci., 1933, 79, 735-745.
9. Cattell, R. B. Temperament tests. II. Tests. Brit. J. Psychol., 1933, 24, 20-49.
10. _____. On the measurement of "perseveration." Brit. J. educ. Psychol., 1935, 5, 76-92.
11. _____. Perseveration and personality: Some experiments and a hypothesis. J. ment. Sci., 1935, 81, 151-167.
12. _____. Temperament tests in clinical practice. Brit. J. med. Psychol., 1936, 16, 43-61.

13. Cattell, R. B. A Guide to Mental Testing. London: University of London Press, 1936, pp. 207-223.
14. _____. The riddle of perseveration. I. "Creative effort" and disposition rigidity. J. Personality, 1946, 14, 229-238.
15. _____. The riddle of perseveration. II. Solution in terms of personality structure. J. Personality, 1946, 14, 239-268.
16. _____. Description and Measurement of Personality. Yonkers: World Book Co., 1946, pp. 433-442.
17. Clarke, G. Some character traits of delinquent and normal children in terms of perseveration factor. Aust. Coun. educ. Res. Ser., 1934, No. 29. Melbourne: Melbourne University Press, 1934.
18. Collins, Jeanne. Perseveration: Its correlation with the intelligence quotient. Master's Thesis, Loyola University Library, Chicago, 1946.
19. _____. The correlation between perseveration test scores and the intelligence quotient: An experimental study. J. genet. Psychol., 1948, 72, 47-55.
20. Culler, A. J. Interference and adaptability. An experimental study of their relation with special reference to individual differences. Arch Psychol., N. Y., 1912, 3, No. 24.
21. Cushing, Hazel M. A perseverative tendency in pre-school children. Arch. Psychol., N. Y., 1929, 17, No. 108.
22. Darroch, Jane. An investigation into the degree of variation in the score of a motor perseveration test. Brit. J. Psychol., 1938, 28, 248-262.
23. _____. A further note on the degree of variation in the score of a motor test of perseveration. Brit. J. Psychol., 1939, 29, 427-428.
24. Dorcus, R. M. Is there a "unit trait" of volitional perseveration? J. gen. Psychol., 1935, 13, 345-356.
25. Downey, June E. The Will-Temperament and Its Testing. Yonkers: World Book Co., 1923, pp. 150-157, 224-225.

26. Eysenck, H. J. Susceptibility to a visual illusion, as related to primary and secondary suggestibility and other functions. Brit. J. Psychol., 1943, 34, 32-36.
27. _____. Suggestibility and Hysteria. J. Neurol. Psychiat., 1943, 6, 22-31.
28. _____. Dimensions of Personality. London: Kegan Paul Trench, Trubner, & Co., 1947, pp. 154-158.
29. Foster, W. S. On the perseverative tendency. Amer. J. Psychol., 1914, 25, 393-426.
30. Garnett, J. C. M. General ability, cleverness, and purpose. Brit. J. Psychol., 1919, 9, 345-366.
31. Gross, O. Die Cerebrale Sekundaerfunktion. Leipzig: Vogel, 1902.
32. Hamilton, N. Perseveration and stability in school children. Unpublished Thesis, Univ. of Sydney Library. Reported by Walker et al. (88).
33. Hargreaves, H. L. The 'faculty' of imagination. An enquiry concerning the existence of a general 'faculty,' or group factor, of imagination. Brit. J. Psychol. Monogr. Suppl., 1927, 3, No. 10.
34. Heymans, G. Ueber einige psychologische Korrelationen. Z. angew. Psychol., 1908, 1, 313-381.
35. _____. and Brugmans, H. Intelligenzpruefungen mit Studierenden. Zschr. f. angew. Psychol., 1913, 7, 317-331.
36. _____ and Wiersma, E. Beitrage zur speciellen Psychologie auf Grund einer Massenuntersuchung. Z. Psychol., 1906, 42, 81-127, 258-301; 1906, 43, 321-373; 1907, 45, 1-42; 1908, 46, 321-333; 1908, 49, 414-439; 1909, 51, 1-72.
37. Howard, C. C. Speed and accuracy as related to intelligence and perseveration. Unpublished Doctor's Dissertation. The University of London Library, London, 1931.
38. Jasper, H. H. Is perseveration a functional unit participating in all behavior processes? J. soc. Psychol., 1931, 2, 28-51.
39. Jersild, A. T. Mental set and shift. Arch. Psychol., N.Y., 1927, 14, No. 89.

40. Jones, Ll. Wynn. Perseveration. Rep. Brit. Assoc. Adv. Sci., 1915, 698-699.
41. _____. An investigation into the significance of perseveration. J. ment. Sci., 1928, 74, 653-659.
42. _____. Individual differences in mental inertia. J. Nat. Inst. Industr. Psychol., 1929, 4, 282-294.
43. _____. An Introduction to Theory and Practice of Psychology. London: Macmillan & Co., 1934, pp. 190-201.
44. Kelley, T. L., Crossroads in the Mind of Man. Stanford, Calif.: Stanford University Press, 1928, pp. 20-21.
45. Kendig, Isabel & Shevach, B. J. Studies in perseveration. I. A survey of researches in perseveration. J. Psychol., 1937, 3, 223-230.
46. Kendig, Isabel. II. Determining factors in the development of compulsive activity. J. Psychol., 1937, 3, 231-246.
47. _____. III. The upper limen for perseveration and repetition. J. Psychol., 1937, 3, 247-251.
48. _____. IV. Selective perseveration and repetition. J. Psychol., 1937, 3, 253-259.
49. _____. V. Theoretical significance of the perseveration and repetition of conative activities. J. Psychol., 1937, 3, 261-264. (For subsequent articles in the series, see: Shevach, B. J.)
50. Kent, Grace H. & Rosanoff, A. J. A study of association in insanity. Amer. J. of Insanity, 1910, 67, 37-96; 317-390; esp. 323-324.
51. Klopfer, B. & Kelley, D. McG. The Rorschach Technique. Yonkers: World Book Co., 1942, pp. 160-161, 344.
52. Kuehle, E. Perseveration von Vorstellungen und Vorstellungselementen. Z. Psychol., 1926, 12, 65-192.
53. Lankes, W. Perseveration. Brit. J. Psychol., 1915, 7, 387-419.
54. Line, W. & Griffin, J. D. M. The objective determination of factors underlying mental health. Amer. J. Psychiat., 1935, 91, 833-842.

55. Line, W., Griffin, J. D. M., & Anderson, G. W. The objective measurement of mental stability. J. ment. Sci., 1935, 81, 61-106.
56. Mays, L. L. Studies of Catatonia. V. Perseverational tendencies in catatonic patients. Psychiat. Quart., 1934, 8, 728-735.
57. Mueller, G. E. & Pilzecker, A. Experimentelle Beitrage zur Lehre vom Gedachtniss. Z. Psychol. Physiol. Sinnes., 1900, 1, 58-78.
58. Neisser, C. 65 Sitzung des Vereins ostdeutscher Irren-aerzte. Allg. Z. Psychiat., 1894, 51, 1016.
59. Notcutt, B. Perseveration and fluency. Brit. J. Psychol., 1943, 33, 200-208.
60. Passarge, E. Perseveration und Determination. Z. Psychol., 1926, 12, 1-64.
61. Pinard, J. W., Tests of perseveration. I. Their relation to character. Brit. J. Psychol., 1932, 23, 5-19.
62. _____. II. Their relation to certain psychopathic conditions and to introversion. Brit. J. Psychol., 1932, 23, 114-126.
63. Rabin, I. Perseveration in dementia praecox and manic depressive psychoses. Unpublished Doctor's Dissertation. Boston University Library, Boston, 1939.
64. Rangachar, C. Differences in perseveration among Jewish and English boys. Brit. J. educ. Psychol., 1932, 2, 199-211.
65. Rethlingshafer, Dorothy. Relation of tests of persistence to other measures of continuance of activities. J. abnorm. soc. Psychol., 1942, 37, 71-82.
66. Reyburn, H. A. & Taylor, J. G. Factors in introversion and extraversion. Brit. J. Psychol., 1941, 31, 335-340.
67. Rogers, K. H. "Intelligence" and "perseveration" related to school achievement. J. exp. Educ., 1933, 2, 35-43.
68. _____. "Perseveration" in a group of subnormal children. J. exp. Educ., 1934, 2, 301-309.
69. _____. Perseverative tendency in motor activity. J. gen. Psychol., 1934, 10, 455-462.

70. Rogers, K. H. Perseveration. J. ment. Sci., 1935, 81, 138-144.
71. _____. Perseveration and personality. J. ment. Sci., 1935, 81, 145-150.
72. Sen Gupta, K. B. An enquiry into some perseveration tendencies in school children. Unpublished Master's Thesis. The University of Leeds, Leeds, 1928.
73. Shevach, B. J. Studies in perseveration. VI. Methods for the study of sensory perseveration. J. Psychol., 1937, 3, 381-402. (For previous articles in the series, see: Kendig, Isabel & Shevach, B. J.)
74. _____. VII. Experimental results of tests for sensory perseveration. J. Psychol., 1937, 3, 403-427.
75. _____. A note on racial difference in perseveration. J. Psychol., 1938, 5, 271-279.
76. Shipley, W. C. Studies of catatonia. VI. Further investigation of the perseverational tendency. Psychiat. Quart., 1934, 8, 736-744.
77. Spearman, C. E. The Abilities of Man. N. Y.: Macmillan Co. 1927, pp. 270-307; esp. 291-307.
78. _____. The Tenth Maudsley Lecture: The psychiatric use of the methods and results of experimental psychology. J. ment. Sci., 1929, 75, 357-370.
79. _____. Psychology down the Ages. London: Macmillan Co., 1937, Vol. II.
80. Stephenson, W., Studies in experimental psychiatry. I. A case of general inertia. J. ment. Sci., 1931, 77, 723-741.
81. _____. An introduction to so-called motor perseveration tests. Brit. J. educ. Psychol., 1934, 4, 186-208.
82. _____. Perseveration and character. Character & Pers., 1935, 4, 44-52.
83. Strauss, A. A. & Werner, H. Experimental analysis of the clinical symptom 'perseveration' in mentally retarded children. Amer. J. ment. Def., 1942, 47, 185-188.

84. Thompson, Helen. The modifiability of play behavior with special reference to attentional characteristics. J. genet. Psychol., 1943, 62, 165-188.
85. Travis, L. E. & Knott, J. R. Brain potential studies of perseveration: I. Perseveration time to light. J. Psychol., 1937, 3, 97-100.
86. _____. II. Perseveration time to visually presented words. J. exp. Psychol., 1937, 21, 353-358.
87. Villamil, P. La perseveración en la edad del praeprendizaje. Med. d. trab. e hig. indus., 1933, 4, 251-270.
88. Walker, K. F., Staines, R. G., & Kenna, J. C. Is there a general factor of perseveration? Aust. J. Psychol. Phil., 1941, 19, 58-75.
89. _____. p. tests and the concept of mental inertia. Character & Pers., 1943, 12, 32-45.
90. _____. The influence of scoring methods upon score in motor perseveration tests. Brit. J. Psychol., 1945, 35, 51-60.
91. Webb, E. Character and intelligence. Brit. J. Psychol. Monogr. Suppl., 1915, 1, No. 3.
92. Wiersma, E. D. Die Sekundaerfunktion bei Psychosen. J. f. Psychol. u. Neurol., 1906, 8, 1-24.
93. _____. Psychology of dementia. J. ment. Sci., 1930, 76, 1-42.
94. Yule, Ella, The resemblance of twins with regard to perseveration. J. ment. Sci., 1935, 81, 489-501.
95. Zillig, Maria. Experimentelle Untersuchungen ueber Umstellbarkeit. Z. Psychol., 1925, 27, 1-31.

B. PSYCHOGALVANIC RESPONSE

96. Brown, W. S. A note on the psycho-galvanic reflex considered in conjunction with estimates of character qualities. Brit. J. Psychol., 1925, 16, 131-135.
97. Cattell, R. B. The significance of the actual resistances in psychogalvanic experiments. Brit. J. Psychol., 1928, 19, 34-43.
98. _____. Experiments on the psychical correlate of the psychogalvanic reflex. Brit. J. Psychol., 1929, 19, 357-386.
99. Darrow, C. W. Electrical and circulatory responses to brief sensory and ideational stimuli. J. exp. Psychol., 1929, 12, 267-300.
100. _____. Differences in the physiological reactions to sensory and ideational stimuli. Psychol. Bull., 1929, 26, 185-201.
101. _____. Uniform current for continuous, standard unit resistance records. J. gen. Psychol., 1932, 6, 471-473.
102. _____. The relation of the galvanic skin reflex recovery curve to reactivity, resistance level and perspiration. J. gen. Psychol., 1932, 7, 261-273.
103. _____. The palmar galvanic skin reflex (sweating) and parasympathetic activity. Amer. J. Psychol., 1936, 48, 522-524.
104. _____. The equation of the galvanic skin reflex curve: I. The dynamics of reaction in relation to excitation-background. J. gen. Psychol., 1937, 16, 285-309.
105. _____. Neural mechanisms controlling the palmar galvanic skin reflex and palmar sweating. Arch. Neurol. Psychiat., Chicago, 1937, 37, 641-663.
106. _____ & Heath, Lena. Reaction tendencies relating to personality. In Lashley, K. S. Studies in the Dynamics of Behavior. Chicago: University of Chicago Press, 1932, pp. 57-261.
107. _____ & Solomon, A. P. Galvanic skin reflex and blood

- pressure reactions in psychotic states. Arch. Neurol. Psychiat., Chicago, 1934, 32, 273-299.
108. Farmer, E. & Chambers, E. G. Concerning the use of the psychogalvanic reflex in psychological experiments. Brit. J. Psychol., 1925, 15, 237-254.
 109. Fleming, E. G. Personality as revealed by the galvanometer. Amer. J. Psychol., 1927, 38, 128-129.
 110. Haggard, E. A. & Garner, W. R. An empirical test of a derived measure of changes in skin resistance. J. exp. Psychol., 1946, 36, 59-70.
 111. Hoch, P., Kubis, J. F., & Rouke, F. L. Psychogalvanometric investigations in psychoses and other abnormal mental states. Psychosom. Med., 1944, 6, 237-243.
 112. Hunt, W. A. Recent developments in the field of emotion. Psychol. Bull., 1941, 38, 249-276.
 113. Landis, C. Electrical phenomena of the skin (galvanic skin response). Psychol. Bull., 1932, 29, 693-752.
 114. _____. Psychiatry and the "psychogalvanic reflex." Psychiat. Quart., 1932, 6, 262-272.
 115. _____. An attempt to measure emotional traits in juvenile delinquency. In Lashley, K. S. Studies in the Dynamics of Behavior. Chicago: University of Chicago Press, 1932, pp. 263-323.
 116. _____ & De Wick, H. N. The electrical phenomena of the skin (psychogalvanic reflex). Psychol. Bull., 1929, 26, 64-119.
 117. _____ & Hunt, W. A. The conscious correlates of the galvanic skin response. J. exp. Psychol., 1935, 18, 505-529.
 118. Lauer, A. R. Reliability of the galvanic reflex. Amer. J. Psychol., 1929, 41, 263-270.
 119. Linde, E. Ueber das psychogalvanische Reflexphaenomen. Z. Psychol., 1930, 115, 34-90.
 120. Porter, J. P. & Copeland, H. A. Association and psychogalvanic responses during early adolescence. Proc. IX Int. Cong. Psychol., 1929, 349-350. Abstract.

121. Syz, H. C. Psycho-galvanic studies on sixty-four medical students. Brit. J. Psychol., 1926, 17, 54-69.
122. Talenti, C. Sulla valutazione dell'emozionabilità per mezzo della reazione psicogalvanica. Bol. soc. Ital. biol. sper., 1929, 4, 714-718.
123. Thouless, R. H. The technique of experimentation on the psycho-galvanic reflex phenomenon and the phenomenon of Tarchanoff. Brit. J. Psychol., 1930, 20, 219-240.
124. Washburn, Margaret F. & Pisek, Frederica. Galvanometer experiments with revived emotions as a test of emotional and phlegmatic temperaments. Amer. J. Psychol., 1925, 36, 459-460.
125. Wechsler, D. On the specificity of emotional reactions. Amer. J. Psychol., 1925, 36, 424-426.
126. _____ Crabs, Lelah M., & Freeman, R. G. Galvanic responses of pre-school children. J. genet. Psychol., 1930, 38, 203-222.
127. Welch, L. & Kubis, J. Conditioned PGR (psychogalvanic response) in states of pathological anxiety. J. ner. ment. Dis., 1947, 105, 372-381.
128. Woodworth, R. S. Experimental Psychology. N. Y.: Henry Holt & Co., 1938, pp. 276-297.

C. VARIA

129. Allport, G. W. Personality: A Psychological Interpretation. N. Y.: Henry Holt & Co., 1937.
130. Hathaway, S. R. & McKinley, J. C. Booklet for the Minnesota Multiphasic Personality Inventory. Minneapolis: University of Minnesota Press, 1943.
131. Kremer, A. H. The nature of persistence. Stud. Psychol. Psychiat., Cath. U., 1942, 5, No. 8.
132. Likert, R. A technique for the measurement of attitudes. Arch. Psychol., N. Y., 1932, 22, No. 140.

133. Likert, R., Roslow, S., & Murphy, G. A simple and reliable method of scoring the Thurstone attitude scales. J. soc. Psychol., 1934, 5, 228-238.
134. Lindquist, E. F. Statistical Analysis in Educational Research. Boston: Houghton Mifflin Co., 1940.
135. Mann, H. B. & Whitney, D. R. On a test of whether one of two random variables is stochastically larger than the other. Ann. math. Statis., 1947, 18, 50-60.
136. Murphy, G., Murphy, Lois B., & Newcomb, T. M. Experimental Social Psychology. N. Y.: Harper & Bros., 1937.
137. Peters, C. C. & Van Voorhis, W. R. Statistical Procedures and Their Mathematical Bases. N. Y.: McGraw-Hill Book Co., 1940.
138. Thornton, G. R. A factor analysis of tests designed to measure persistence. Psychol. Monogr., 1939, 51, No. 3.
139. _____. How general is the factor of "persistence"? A reexamination and evaluation of Ryans' results. J. gen. Psychol., 1940, 23, 185-189.
140. Thurstone, L. L. & Thurstone, Thelma G. A neurotic inventory. J. soc. Psychol., 1930, 1, 3-30.

APPENDIX I

THE PERSEVERATION QUESTIONNAIRE*

Name _____ Date _____

Date of Birth _____ Class _____

INTRODUCTION: This is an attempt to measure one of the many traits we all possess to some degree. Your cooperation in following instructions carefully will be appreciated.

INSTRUCTIONS: Read each question carefully. Then place a check (✓) before the corresponding descriptive term which is true for you. If none of the terms seems to fit exactly, check the one which is closest to the truth.

In a scale of this kind, the only true answer is the truthful one. Please be frank. Keep in mind that what you consider a damaging admission may actually be a desirable quality.

If you are in doubt about the meaning of any question, ask the instructor to explain. Then if you are still in doubt, write a question mark next to the number of the question, answer it as well as you can, and proceed to the next question. After you have answered them all, you will be given ample time to describe any difficulty, make explanations, or add remarks on the back of the last page. These comments will be helpful to the experimenter.

Please answer every question.

1. When you are writing an essay or trying to solve a problem, do you find it hard to lay it aside for interruptions? (By "problem" is not meant a personal problem or anything that is a source of worry.)
(2) Occasionally. (1) Seldom. (5) Almost always. (0) Never.
(3) Often. (4) Very often.

* This title did not appear on the questionnaire as actually used.

2. If you have laid aside a problem of this kind, do you find that it still keeps coming back to your mind?
(5) Almost always. (3) Often. (2) Occasionally. (4) Very often. (0) Never. (1) Seldom.
3. After a trip by boat, train, car, or other vehicle, do you seem to keep on hearing the noise or feeling the motion for a time?
(1) Seldom. (0) Never. (4) Very often. (5) Almost always. (2) Occasionally. (3) Often.
4. Do these sensations later come back in your dreams?
(4) Very often. (2) Occasionally. (1) Seldom. (3) Often. (5) Almost always. (0) Never.
5. Does it usually annoy you to have many different tasks or duties to look after?
(3) Much. (4) Very much. (0) Not at all. (2) Somewhat. (1) A little bit. (5) So much that I worry and get anxious.
6. Do you dream at night?
(0) Never. (5) Nearly every night. (3) 2 or 3 times a month. (1) Not more than 5 or 6 times a year. (4) About once a week. (2) About once a month.
7. Do you dream about things that have recently happened?
(5) Practically whenever I dream. (1) Seldom. (2) Occasionally. (0) Never. (4) Very often. (3) Often.
8. Do tunes keep running through your mind without the least effort or intention on your part?
(3) Often. (2) Occasionally. (5) Constantly. (4) Very often. (0) Never. (1) Seldom.
9. When unexpectedly addressed or asked a question which you know well enough, but have not been thinking of at the time, can you answer easily and quickly?
(0) Practically always. (5) Practically never. (3) Not quite half the time. (2) More than half the time. (1) Usually. (4) Generally not, but sometimes.
10. Do you like changes in the routine of life?
(2) Like routine in regard to certain things only. (4) Rarely like a change in routine. (1) Like a little routine; but the less, the better. (3) Like routine for most of my day, with some room for variety. (5) Any change is a nuisance. (0) Prefer no routine whatsoever.

11. When you become angry, do you get over it fairly quickly?
(4) Seldom. (3) Occasionally. (0) Almost always. (5) Never.
(1) Very often. (2) Often.
12. Do you worry about things?
(1) Seldom. (0) Hardly at all. (4) Very often. (2) Occasionally. (3) Often. (5) Extremely often.
13. If you have some worry, does it keep coming back to mind when you don't want to think of it? (If you have no worries, check "Never").
(4) Very often. (1) Seldom. (3) Often. (0) Never. (5) Constantly. (2) Occasionally.
14. When you turn back to a task after a brief interruption, can you get the task back into mind readily, so that the former thoughts come back easily? (I.e., you feel you "have your bearings" at once.)
(2) Usually. (5) Very seldom. (1) About 9 times out of 10.
(4) Generally not. (3) About half the time. (0) Always.
15. Do you get over a disappointment very quickly?
(1) Very often. (3) Occasionally. (0) Practically always.
(2) Often. (4) Seldom. (5) Never.
16. Do you dream about things that worry you? (If you have no worries, check "Never".)
(5) Constantly. (2) Occasionally. (4) Very often. (1) Seldom. (0) never. (3) Often.
17. When you have seen a very tragic play or movie, does the emotion linger with you for hours afterwards?
(3) Often. (0) Never. (2) Occasionally. (5) Practically always. (1) Seldom. (4) Very often.
18. Do lines of poetry, words, or phrases spontaneously keep coming to your mind?
(1) Seldom. (0) Never. (3) Often. (4) Very often. (2) Occasionally. (5) Constantly.
19. After you have lived in one room or place for some time, do you find it hard to settle down to work in new quarters?
(0) Not at all. (5) Extremely hard. (4) Very hard. (2) A bit hard. (3) Noticeably hard. (1) Not worth mentioning.
20. If a little thing goes wrong early in the day, does it put you in a bad mood?
(4) Very often. (3) Often. (1) Seldom. (5) Practically always. (0) Never. (2) Occasionally.

21. When you have an important or somewhat unfamiliar task ahead of you and the day for it approaches, do you catch yourself thinking about it even when you don't want to?
(5) Extremely often. (4) Very often. (2) Occasionally.
(0) Never. (1) Seldom. (3) Often.
22. Are you aware of being bothered by unimportant or useless thoughts or ideas that keep coming back to your mind?
(3) Often. (2) Occasionally. (0) Never. (1) Seldom.
(5) Extremely often. (4) Very often.
23. Do you find it hard to shake off a spell of the blues?
(2) Occasionally. (1) Seldom. (5) Extremely often.
(3) Often. (4) Very often. (0) Never.
24. Are you generally able to keep your mind on a task or job?
(3) Easily. (1) With great difficulty. (4) Very easily.
(5) Quickly become completely absorbed in it. (0) Yes, but with very great difficulty. (2) With moderate difficulty.
25. When you awaken during a dream, does it continue when you are asleep again?
(4) Very often. (5) Almost always. (2) Occasionally.
(0) Never. (1) Seldom. (3) Often.
26. When you cannot recall a name, does it disturb you until you can recall it?
(5) Almost always. (0) Never. (3) Often. (1) Seldom.
(2) Occasionally. (4) Very often.
27. Can you change from one activity to another readily?
(1) Very often. (4) Seldom. (5) Never. (2) Often.
(3) Occasionally. (0) Practically always.
28. When you are asked a question you cannot answer, does it bother you afterwards until you have the answer?
(2) Occasionally. (3) Often. (0) Never. (4) Very often.
(5) Practically always. (1) Seldom.
29. Do you ever carry out an activity somewhat automatically, having temporarily forgotten the purpose of the act?
(5) Extremely often. (3) Often. (1) Seldom. (4) Very often. (2) Occasionally. (0) Never.
30. Do you fall easily into a steady routine without giving it any particular thought or effort. (E.G., doing the same things at the same time day after day.)
(2) Occasionally. (0) Never. (4) Very often. (5) Practically always. (1) Seldom. (3) Often.

31. Do you like to dwell on ideas, turning them over and over in your mind and examining them from all angles?
(3) Often. (1) Seldom. (2) Occasionally. (0) Never.
(5) Constantly. (4) Very often.
32. Do you prefer to stick to a task until it is finished, rather than do just a part of it at a time?
(4) Very often. (2) Occasionally. (3) Often. (1) Seldom.
(0) Never. (5) Practically always.
33. When you are reading something interesting, do you find it hard to lay aside for awhile?
(0) Never. (4) Very often. (5) Practically always.
(1) Seldom. (3) Often. (2) Occasionally.
34. When you plan something, do your plans keep coming back to mind, even though they are complete and you are not afraid you have overlooked something?
(1) Seldom. (5) Practically always. (0) Never. (3) Often.
(4) Very often. (2) Occasionally.
35. In conversation, do you find that one thing leads to another and you tend to get off on some other subject?
(1) Very often. (3) Occasionally. (2) Often. (4) Seldom.
(5) Never. (0) Extremely often.
36. Do you do better by thinking straight through a problem from start to finish, rather than by frequently dropping it so as to take it up again later?
(3) Often. (1) Seldom. (0) Never. (2) Occasionally.
(4) Very often. (5) Invariably.
37. When you are in a very good mood and things seem rosy, do you tend to stay that way for some time, despite minor difficulties and troubles?
(0) I change for no apparent reason. (4) Very serious matters upset me. (3) Serious matters upset me. (5) Nothing can upset me. (1) Trivial things upset me. (2) Minor difficulties upset me.
38. Do you day dream?
(2) Occasionally. (0) Never. (5) Extremely often.
(1) Seldom. (3) Often. (4) Very often.

39. Do you prefer to do one task at a time and finish it before going on to another, rather than to have several "irons in the fire" at the same time?
(4) Very much prefer. (5) Cannot stand more than one at a time. (1) Slightly prefer. (0) Do not at all prefer.
(2) Somewhat prefer. (3) Much prefer.
40. Do you find that you seem to pick up the latest slang at once and automatically, without particularly wishing to do so?
(5) Practically always. (2) Occasionally. (4) Very often.
(3) Often. (0) Never. (1) Seldom.

APPENDIX II

TABLE XXI

SOME COLLECTED DATA

Subject	Perseve- ration	Recovery Rating	Recovery Per Cent	Average Drop	Resistance Level
1	26	.83	11.8	293.8	26,885
2	33	-1.19	85.8	313.2	12,150
3	36	-1.56	65.7	809.3	20,688
4	38	-1.00	63.9	272.5	15,844
5	39	-1.56	67.0	546.1	18,700
6	41	.18	53.3	207.1	15,375
7	42	-.15	59.5	483.4	17,806
8	42	-1.19	59.3	562.9	16,000
9	43	-1.56	73.3	687.2	20,425
10	44	-1.56	71.7	449.2	21,292
11	45	-.44	52.2	392.9	17,321
12	46	.34	37.0	524.7	20,731
13	46	.20	44.9	229.5	18,125
14	47	-.29	50.5	473.5	15,818
15	48	-.44	48.3	412.6	22,093
16	49	.49	29.0	287.0	21,781
17	49	1.35	41.1	181.8	18,643
18	49	.03	48.8	236.1	17,222
19	49	.83	20.2	318.3	22,229
20	49	-.29	40.7	523.9	22,333
21	50	.83	--	--	35,167
22	50	.03	44.2	526.8	16,000
23	51	1.35	30.5	225.3	18,393
24	52	.03	37.1	660.4	31,063
25	53	.03	67.5	180.9	18,000
26	54	.20	32.6	630.7	25,792
27	55	.66	23.6	319.0	19,458
28	55	.18	50.0	341.5	24,219
29	55	.18	32.0	369.0	16,792
30	55	-.29	42.5	444.6	22,625
31	57	-.44	57.1	391.6	15,014
32	57	-1.19	83.2	298.5	24,063

TABLE XXI (CONT.)

Subject	Perseve- ration	Recovery Rating	Recovery Per Cent	Average Drop	Resistance Level
33	58	-.44	57.1	329.6	21,000
34	59	1.52	--	---	22,500
35	60	-.44	58.0	394.5	11,865
36	61	.18	46.3	269.9	20,406
37	61	-1.00	72.6	339.5	17,432
38	62	-.81	72.1	331.1	16,575
39	62	.49	30.1	345.3	17,556
40	62	1.01	17.4	684.3	20,417
41	62	1.52	33.9	388.9	19,194
42	64	-.29	41.4	527.5	14,025
43	64	.53	50.5	240.6	12,938
44	64	-.29	57.3	283.3	17,611
45	66	1.52	25.4	392.3	27,313
46	67	.49	17.6	562.7	22,250
47	68	-1.37	70.0	653.9	26,314
48	71	.18	50.5	125.8	10,250
49	72	.03	39.2	309.4	15,438
50	74	1.18	26.6	318.2	33,500
51	79	1.18	16.5	352.6	17,063
52	81	.20	53.5	183.9	18,107

N. B.: Only those data were included in the table which were thought helpful to one who may wish to consider relationships other than those taken up by the present writer. Other data mentioned in the body of the dissertation but not reproduced here may be obtained upon request.

APPROVAL SHEET

The dissertation submitted by Charles A. Weisgerber, S.J. has been read and approved by five members of the Department of Psychology.

The final copies have been examined by the director of the dissertation and the signature which appears below verifies the fact that any necessary changes have been incorporated, and that the dissertation is now given final approval with reference to content, form, and mechanical accuracy.

The dissertation is therefore accepted in partial fulfillment of the requirements for the Degree of Doctor of Philosophy.

Dec. 25, 1950
Date

V.V. Hennig
Signature of Adviser